absence of any mention of the great Polish school of methodology which flourished between the two world wars and contributed so much to the clarification of the problems discussed in these essays and greatly influenced some members of the Vienna Circle. The names of Kotarbiński, Leśniewski, Lukasiewicz and Tarski do not appear in its pages.[1]

Nevertheless this book contains much interesting reading matter and may be warmly recommended to students of the history of science.

J. H. WOODGER

The Revolt Against Reason, A. Lunn, Eyre & Spottiswoode (Publishers), Ltd., London, 1950. Pp. 252.

THE taste for controversial literature, widespread in the seventeenth and eighteenth centuries, has so far diminished now that a would-be controversialist, desiring to woo the attention of the reading public, has usually to disguise his work as a novel or a scientific or philosophic treatise. Mr Lunn, however, disdains any such subterfuge ; he writes as a controversialist with a point to make, namely that materialism, whether vulgar or dialectical, behaviourism and logical positivism are destructive of their own logical coherence, and entail a simultaneous breakdown in moral values.

Though his theme is single, Mr Lunn treats its subordinate aspects rather unevenly. The earlier chapters of his book outline the birth of scientific inquiry and the development of the scholastic method that formed the matrix of scientific ratiocination. The attack by Luther on reason is then described together with the concurrent moral disorders of the German Reformation period. Several chapters are devoted to the rise within science of an anti-religious bias, which gained so greatly from post-Darwinian evolutionary thought, and in this connection Mr Lunn includes an excursus on the evidences for biological evolution by which he is not greatly impressed. As a counter-blast to scientific materialism, a chapter is introduced on the evidences provided by psychical research, the author remarking, correctly enough, that these are more likely to interest inquirers today than philosophic arguments against materialism, however closely reasoned. The final chapters deal rather cursorily with materialist psychology, the treatment of science in the Soviet Union, the less pleasant traits of surrealism and logical positivism.

It is, of course, impossible in one book to deal exhaustively with all these topics. Mr Lunn, however, does succeed in his main objective of exposing some of the logical inconsistencies in the views he combats, and of pointing out how they subvert morality. Another rather unpopular point which the author makes, is the extent to which the philosophical, religious or anti-religious views of scientists colour their own scientific writings. Only those with no scientific contacts can minimise the force of this observation. Mr Lunn is certainly correct, too, in claiming that many of the views he combats have gained popularity through their approbation of departures from traditional morality.

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[1] A good account of their work is given in *Polish Science and Learning*, No. 6, Oxford University Press, 1945, price 2s. 6d.

Mr Lunn is at his best when dealing with general principles. His treatment of more concrete issues tends to be prejudiced by insufficient cover of the relevant data. Modern evolutionary theory, for instance, is largely based on genetic grounds. Mr Lunn does not consider these, though he refers repeatedly to the dicta of various scientists, including Darwin himself. Evolution, however, is an induction from evidence and it is quite irrelevant to consider personal views, many of which, including Darwin's, are quite obviously false and of no more than historic interest. As regards the objections that Mr Lunn puts forward against evolution, it is relevant to observe (1) that the choice between asexual and sexual reproduction may be determined by a simple gene difference in many higher plants, (2) that complex organs are usually regarded as having evolved from more rudimentary organs, not from undifferentiated tissues, (3) that single mutants have been observed so fundamental that were their origin unknown they would have been classified in a separate family from their parents ; (4) that while flight may be a ' hit or miss ' phenomenon, the structural modifications that led up to it may have been relatively gradual ; (5) that nascent bony structures are known from the fossil record ; and (6) that the affinities of *Archaeopteryx* with both reptiles and birds cannot be dismissed by a quotation from Berg.

Mr Lunn's book would also benefit from a more thorough proof reading, and in this connection surely ' how ' and ' why ' have been mixed up in the fourth paragraph on p. 68.

R. H. RICHENS

The Philosophy of Mathematics, Edward A. Maziarz, Philosophical Library, New York, 1950. Pp. viii + 286. $4.00.

THE author of this book means by the philosophy of mathematics the ' field of philosophical enquiry concerned with the nature of mathematical abstraction.' In an introductory chapter he seeks to place the subject ' in a wider speculative setting.' The rest of the book is then in two parts.

Part I is on ' The history of the philosophy of mathematics.' This again starts with an introductory survey on the historical relation between mathematics and philosophy. The following chapters deal in turn with ancient mathematics and philosophy, the ' Cartesian era,' the British empiricists, the idealism of Kant and the positivism of Comte, and finally, in more general terms, with modern trends.

Part II is called 'The philosophy of mathematics.' In its first chapter the author presents a classification of knowledge in general and then proposes to classify the 'speculative sciences' into *physical science* characterised by 'total abstraction,' *mathematical science* characterised by 'formal abstraction,' and *metaphysics* as 'the most perfect manner of knowing of which the human mind is capable.' The next two chapters deal more fully with mathematical abstraction. Here and elsewhere in the book there are brief commentaries upon the three contemporary views of the nature of mathematics—logicist, formalist, and intuitionist. But the author is mainly concerned with what he means by mathematics as an abstractive science and the consequent status of what he calls the 'mathematical universe.' So far as I understand him, his attitude is simply that mathematics has a foundation in man's common experience of an external world, that it is reached by one particular kind of activity of the mind in response to this experience, that this kind of mental activity is distinguishable from, but related to, other discernible kinds, and that there is a unifying activity which is metaphysics. Together with reiterated criticisms of tendencies to regard mathematics either as being without foundation in extra-mental experience or as being more comprehensive than he allows, this I think is the position summarised in the author's concluding chapter. He expresses it, however, in very different language.

At the outset one becomes sympathetically disposed towards the author because of the evident sincerity of his conviction that he has something worth saying, and because he expresses himself with such spontaneous fluency. Also, what appears to be his main thesis, that an adequate philosophy of mathematics is to be attained only by due regard to the place of mathematics in thought in general, and his implied view of the significance of the historical background are unexceptionable.

Unfortunately, the sympathetic reader is likely to be disappointed later on. For the book turns out to be rather one *about* the philosophy of mathematics than an exposition *of* the subject. The author does not get down to any systematic analytical study. Indeed, so far as I can penetrate what is to a mathematician a haze of metaphysical terms, the author's views upon the nature of physical science and of mathematics appear to verge upon the naive. Also he seems to employ only preconceived and undefined meanings for many of his general notions such as 'knowledge,' 'reality' and 'existence.'

Nevertheless, no review of the book should fail to praise the care with which it has been documented. With its copious references and bibliography it can form a valuable guide to the literature of the vast field with which it deals.

W. H. McCrea

A Critique of Logical Positivism, C. E. M. Joad, Victor Gollancz, Ltd., London, 1950. Pp. 154. 10s. 6d.

During the last twenty years the so-called logical positivist has risen in the philosophical world from *enfant terrible* to whipping boy. No one quite knows what is meant by ' Logical Positivism '; but everybody is against it. This makes the philosopher's game more amusing since he can so set up a scarecrow as adversary and knock it down again to display his prowess. Dr Joad plays this game with gusto, taking the first edition of Ayer's *Language, Truth and Logic* as a specimen of Logical Positivism.

There is no doubt that the ideas contained in that book require criticism, some of which Professor Ayer has indicated in the preface to the second edition. Dr Joad's critique can thus be only a post mortem—an historical task ; and to assess his critical efforts would need detailed confrontation with Ayer's book. But what makes Joad's essay of general interest is that it transcends the original objective.

Historically, Logical Positivism was the attempt to apply modern logic and scientific method to the problems of philosophy, a successor to the older kind of empiricism as Joad points out. This is, of course, why Logical Positivism is unsatisfactory today ; it remained within the confines of traditional epistemology. When it was seen that scientific method tolerates no -isms, the early formulations of the Vienna circle (usually spoken of as Logical Positivism) were changed. This illustrates the scientific attitude of always revising and, if possible, of improving our theories. Curiously enough it appears to irritate Dr Joad. While imputing dogmatism to the logical positivists, he complains that they have now re-formulated their main statements so as to make them nearly acceptable. It is true that the original pronouncements of logical positivists were needlessly provocative ; but this is not the reason why the mere mention of ' Logical Positivism ' makes people see red.

It is that ' under its influence young men and women confidently affirm that there are no absolutes, that metaphysics is nonsense, that the scientific is the only method which reaches valid results and that the order of reality which science studies is the only order that there is ' (p. 10).

And here is the main reason for Joad's, and everyone else's, attack (and the only reason for taking the trouble of reviewing this book) : the dislike of logic and of science. It is shown also by the fact that the three main ideas under attack are exactly those which, though certainly in need of a more precise formulation, represent genuine achievements closely related to modern science. To put it briefly : (1) The verifiability criterion of meaning is surely acceptable though its epistemological form must be abandoned. It is neither a thesis, nor a principle, nor a law, nor a prohibition, not even simply a definition ; but an attempt of reconstructing in logical terms the use of factual statements, both within the context of science and of everyday life. There is a problem of adequacy, but not of truth or of metaphysical principle ; the discussion in Joad's book does not make this clear. (2) The rejection of metaphysics, I agree, is sometimes made in too sweeping a manner—for the reason that metaphysical sentences are so obscure and metaphysicians differ so much with one another. Yet, as Dr Joad correctly says, certain metaphysical sentences are claimed as factual but cannot be verified by experience. That is to say, we are expected to accept sentences about matters of fact that are, in principle, not verifiable by any fact. We are asked to use sentences for which no (semantic) rules of usage can be given. It is no wonder that logical or factual meaning cannot be ascribed to such sentences, when taken literally. It does not follow, however, that sentences of this sort are unimportant ; they have often an enormous emotional import (e.g. in theology), and this has never been denied. (3) Finally, the emotive theory of ethics is claimed to undermine morality and lead to Fascism. 'Logical positivist conclusions really do eviscerate the universe' (p. 19). This furnishes the recurrent theme of Joad's book. It is a touchingly old-fashioned argument for which, however, I am grateful to Dr Joad that, today, he still feels concern about Fascism. But since 399 B.C. this kind of reasoning has often been used against philosophic radicals and, as one must admit, not always with success. For historical accuracy it must be said that the Continental adherents of what is loosely called Logical Positivism have been, as far as I know without exception, political refugees from Fascism ; and today their writings are banned in Soviet Russia. One could not wish for a better recommendation.

But to come to a more serious argument : Dr Joad says that to accept the emotive theory 'is to deprive both ethics and religion of emotive significance' (p. 145). In other words, to say that ethical sentences are primarily used to express emotion is to say that they are emotionally not significant ! The emotive theory is anyway not quite so simple since ethical sentences are used within a context and so are related to factual sentences. The theory emphasises the different usage to which we put ethical and factual sentences and attempts to analyse the meaning of the terms involved. To investigate what we mean by 'right' and 'wrong,' or by 'value,' does not imply that we must not use these terms nor that sentences containing them have no import. To say 'Do not kill' is not to describe anything but to exhort us to act in a certain manner, and so the problem of truth does not arise ; not even in the sense of the Correspondence theory of truth which Joad holds. For who would affirm that we accept this command because it corresponds to a fact ?

No, this won't do. Dr Joad does not analyse Professor Ayer's views, nor lay the ghost of Logical Positivism. Meaning analysis with the help of logic and of scientific method is condemned, on emotional grounds. What a pity that Dr Joad did not heed the advice of the motto which he has made his trade-mark ' It all depends what you mean.'

E. H. HUTTEN

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Inclusion of books in this list does not preclude their being reviewed in later issues

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THE RELATION BETWEEN THE TIME OF PSYCHOLOGY AND THE TIME OF PHYSICS

PART II *

(3) *The Physical Counterpart to the Specious Present*

As I remarked earlier, there is implicit in this paper a theory of psychoneural parallelism in regard to the relation between experiential events, happening to someone's mind, and the physical events, which a physiologist would say were happening to that person's body, and (more particularly were) occurring in his brain at that time. It is impossible here to set out adequately my reasons for holding such a view. I would like only to say two things to indicate roughly what my standpoint is. First, I am adopting a standpoint very similar to the one expressed by Professor C. Burt, in his book *The Factors of the Mind*,[1] on pp. 232 to 233 of which he says :

> I would argue that, whether we are studying the material world as physicists or the world of experience as psychologists, the only articulate or communicable knowledge that we can attain is a knowledge of structure. Let me add that, if this position be accepted, the relation of matter and mind would lose much of its mystery, for we should no longer be concerned with the interactions of disparate substances but with the correspondence of abstract structures.

Secondly, another reason for adopting such a psychoneural parallelism, is, I would say, that factor analysis is one of the branches of psychology which have shown substantial progress in the experimental field, and have produced results enjoying a reasonable degree of confirmation, upon repetition, in experiments by a number of independent investigators. Another branch of psychology, which has been outstandingly successful experimentally, the Gestalt theory, also takes up a similar standpoint in its postulation of an ' isomorphism ' between psychological and physical structures. Such parallelism would seem, therefore, to be a sufficiently valuable heuristic postulate ; and without further ado I now turn to consider the physical counterpart to the experiential phenomenon of the specious present.

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* Part I appeared in Volume 2, no. 6 of this *Journal.*

[1] London, 1940

I have indicated earlier that I believe the physical counterpart of the relation of ' compresence,' found in experiential events, is the relation of superposition used in the current quantum theory of physical events. And the temporal uncertainty, or indeterminacy, which is associated with this relation of superposition in certain kinds of physical events, is the isomorph of the spread of compresence within one speciously present experiential event.

The distinctively non-classical quantum-superposition relationship holds, according to quantum theory, between the basic states of a dynamical system. It is different from the classical notion of superposition of figures in Euclidean geometry and of vector components according to classical physics. Dirac has expressed this difference by stressing the peculiarly indeterminate, or intermediate, character of a state of a system formed by the process of quantum superposition. He says in his *Principles of Quantum Mechanics* : [1]

> It is important to remember, however, that the superposition that occurs in Quantum Mechanics is of an essentially different nature from any occurring in the classical theory as is shown by the fact that the Quantum superposition principle demands indeterminacy in the results of observations in order to be capable of a sensible physical interpretation.

These words make it quite clear that the distinctive feature about the notion of a superposition in quantum physics, as opposed to classical physics, is indeterminacy ; and this indeterminacy is a necessary result of the process whereby, 'When a state is formed by the superposition of two other states, it will have properties that are in some vague way intermediate between those of the two original states and that approach more or less closely to those of either of them. . . .' [2]

We owe to Heisenberg the precise formulation of the uncertainty principle governing the indeterminacy in the numerical absolute values of the conjugate variables describing a physical system compounded by superposition of states with incoherent phase. Heisenberg's principle is often formulated as $\Delta p \, . \, \Delta q \sim h$ where 'p' and 'q' represent any pair of variables canonically conjugate in the quantum theory analogue of Hamilton's equations for the motion of the system (giving its change in the course of time), and 'h' is Planck's constant. The particular application of this relation which interests us here is to the pair of conjugate variables used to represent the quantities energy and time as they are at present conceived in physics. This application of Heisenberg's principle can be written $\Delta w \, . \, \Delta t \sim h$. As a result of these uncertainty relations, it is not possible to specify a 'state' of a physical system with the kind of accuracy conceived in classical physical theory, according to which it was supposed that, in the last analysis, it would prove possible to assign simultaneously numerical values to *all* the variables descriptive of the system.

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[1] 2nd ed., Oxford, 1935, p. 14

[2] Ibid. p. 12

This means that when a sub-atomic or microscopic physical system is subject to a disturbance, as, for instance, when a piece of matter (e.g. an atom or molecule) interacts with a field of radiation (as happens, according to physics, when a flash of light falls upon the human eye) the resulting changes in the energy levels of the system, consisting in absorption of certain frequencies of electromagnetic waves, can only be 'specified by fewer or more indefinite data than a complete set of numerical values for all the coordinates and velocities at some instant of time.'[1] Such a situation must be conceived as a superposition of a set of states, with different coordinates and momenta, distributed throughout a range of point-instants constituting a region of space-time of *finite* extent. When a sharply defined *space*-region is considered, there is a corresponding uncertainty about the velocities of the occupants of that region; and, as far as a sharply defined energy change is concerned, there is a corresponding uncertainty about the exact *instant* at which classical physics would say the occurrence happened: the most that can be said according to quantum physics is that the changes occur in a range of overlapping states combined by superposition.

When these notions are translated into Russell's and my language for the description of the phenomenology of the specious present, they are clearly equivalent to the idea of a range of different physical states or event-phases being compresent together in a given region of space-time, where there is interaction between matter and radiation. Of course, the order of magnitudes involved, according to current physical views, is very small. For example, the period of a light wave is of the order 10^{-15} seconds ; and, if we try to measure times to this order of accuracy, we are faced with uncertainty in the energy of the order of one electron-volt. But, at any rate, so far as a sub-atomic system is concerned, we can say that quantum theory envisages a state of affairs which has a definite analogy to the specious present in psychology. In both cases we have occurrences between which relations of 'succession' or 'precedence,' in one time-order, can be seen to hold. And yet this succession or precedence is *not* incompatible with taking the set, in another sense (i.e. in respect of another temporal order), as a *compresent whole :* i.e. it is as if the whole set were compresent together, so that it is not correct to suppose some of the elements of it have ceased to exist before the others began.

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[1] Dirac, op. cit. p. 11

This can best be seen, so far as quantum phenomena go, if we consider the case of electrons in a simple atomic system, such as the steady states of the hydrogen atom. These electrons are, according to the Rutherford-Bohr atomic theory, to be conceived as revolving in orbits round the nuclei of the atoms concerned. Quantum theory does not now suppose these states to be like this ; but it does in general ascribe to them an angular momentum (either spin or orbital or both) which can only be intelligibly conceived on the supposition that there is *some* form of internal circulation within each state, involving the idea of a succession of spatio-temporal locations and transformations. Otherwise, there could be no meaning in talking of *angular momenta* or of *motion* at all.

No such logical difficulty arises in connection with classical theories of the atom, such as the classical Rutherford-Bohr theory, according to which, as we have said, these successive phases of steady states of angular momenta are conceived in terms of the successive occupation by the electrons of different regions of space, at different instants of time, between which relations of temporal precedence subsist and recur periodically. Though in the Bohr model it became necessary to admit the occurrence of transition between steady states in which it was necessary to suppose that an electron could move from position $x_0\ y_0\ z_0$ to position $x_1\ y_1\ z_1$ without occupying intermediate positions at any instant of time. In any case, the Rutherford-Bohr theory could not account for the most fundamental of the empirical facts ; such as the peculiar stability of the atom's states, and the fine structure of the lines in its emission or absorption spectrum. It was to overcome just these empirical shortcomings that the quantum physics of Heisenberg and Schrödinger was introduced.

But the introduction of the ' new-look ' quantum physics in general, and of Heisenberg's uncertainty principle in particular, has profoundly changed the basis of the classical conception. For, as a result of Heisenberg's uncertainty principle, it is no longer possible to think of any definite energy-level as being observable at a precise instant of time. The point is well brought out in the following passage from an eminently practical book, on the technological consequences for metallurgy of the newer quantum physical ideas, by William Hume-Rotheray. He stresses the impossibility of looking within the process of circulation by an electron in a stationary state of an atomic system, so that such a state can only be represented by regarding all its positions as being in a way compresent together in a kind of spatio-temporal cloud. As he remarks ; ' In all these electron cloud patterns you have to accept the pattern as a whole, and to recognise that the pattern itself is as far as we can go in providing a model or picture of what the electron does. . . .'[1]

Despite, however, its rejection of the idea of the possibility of observing a steady energy level of a physical system at a single definite instant, we still, as we have already said, find that quantum theory employs the notion of angular momentum in connection with the states of electrons or energy levels of a system. Indeed, it has been said that ' it will be possible to confirm that to any electronic state there is to be ascribed a continuous flow similar to the circulation of an electron in an orbit.'[2] This means that though such states of motion must be considered as wholes, yet they must also be supposed to have an internal structure comprising conceptually discriminable and periodically recurrent phases. The whole range of such phases, constituting a single period, must be taken together as a compresent whole ; and so it is *not* permissible to ask at what particular times their successive phases were present.

I must pause here to remove a possible misconception of what I am saying. I am not referring to the kind of question which can always be raised about motion both in classical and quantum theories and which was first suggested, according to Aristotle, by Zeno the Eleatic in the fifth century B.C. This question Zeno put in the form of his famous ' flying arrow paradox.'[3] The paradox arises from the fact that on any view of motion it is always possible to regard it, or any process of change for that matter, from two aspects ; (*a*) as a dynamic process, (*b*) as a static instantaneous condition. And in principle classical physics assumes that there are two, correspondingly different, ways of measuring motion : (i) by taking *two* distinct observations and recording the distance travelled in the time elapsed between them, and (ii) by taking *one* reading of an instrument (such as a speedometer) which works by recording statically the differential effects of different degrees of motion.

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[1] *Electrons, Atoms, Metals and Alloys*, London, 1948, p. 74

[2] R. W. Gurney, *Elementary Quantum Mechanics*, Cambridge, 1940, p. 30

[3] Aristotle, *Physics*, VI. 9, 239 b. 5 ff.

But this classical question about motion is *not* the one we are discussing now. The classical question is essentially concerned with, and can be answered by, technical mathematical considerations about the nature of continuity. In this regard there is no logical difficulty about motion as long as the mathematical functions used to describe it are continuous, so as to have finite non-vanishing derivatives at all points. In such cases the motion can always be described in *both* of the ways we have mentioned : (*a*) either on the ' instantaneous ' method by specifying the instantaneous spatio-temporal coordinates, together with the first and second derivatives with respect to the time ; or (*b*) on the ' holistic ' method by giving a value to the definite integral of the function within specified limits, provided, of course, that such an integral exists.

But just these considerations show that the quantum paradox we have just been considering is a different one altogether. For the difficulty about the idea of the motion of an electron in a stationary atomic state, conceived according to current quantum theory time-views, is just the *impossibility* of attaching *any* meaning to the ' instantaneous ' interpretation. Formal considerations of continuity cannot help here.

So we find ourselves driven to a conclusion similar to the one we reached in discussing, in Sections (1) and (2), the phenomenon of the specious present. For clearly there is only one way of making sense out of these conditions ; and that is to introduce *two* systems for the ordering of the extensive aspects of temporality in physics. These two time-orders must, in principle, be able to be logically completely independent, as are the three axes in a Cartesian coordinate system. For if it is granted that ' uncertainty of physical time ' may be treated analogously to Russell's notion of compresence with a specious present, it is clearly an empirical accident, happening to obtain in our physical world, that the spread of compresence should be prescribed by the particular numerical value of the quantum of action represented by Planck's constant '*h.*' In principle it should be possible for every conceivable range of 'instants' to be compresent together in a single span of compresence. This can only be if the two time-orders are *logically* and *intrinsically* as completely independent of each other as are the three mutually perpendicular directions, or coordinate-axes, of three-dimensional space in elementary Euclidean 'solid geometry.'

That there is already awareness on the part of physicists of the need to introduce more than one time-like dimension to represent certain aspects of temporality in quantum theory comes out significantly in the many passages in the late Sir Arthur Eddington's *Fundamental Theory.*[1]

Although I do not wish to associate myself with what is perhaps Eddington's main contention in this book : that the 'constants of nature' can *all* ultimately be deduced from an epistemological analysis of the nature of the process of measurement used in physics—a view which I hold to be mistaken—yet I believe there are many features of his approach which mark a profound advance in the phenomenology of physics. Eddington asserts in *Fundamental Theory*, pages 71 to 72, that :

> In statistical mechanics the time is distinguished from the other coordinates at the very outset. 'Probability distribution over time' has no intelligible meaning. . . . But we can obtain a relativistic extension of statistical mechanics by taking as the fourth dimension, not time, but the scale and phase dimension. . . . The scale and phase have, therefore, a time-like relation to the spatial momenta and coordinates.
>
> In 1928 Dirac made his famous relativistic extension of wave mechanics. The *wave-vectors* which he introduced have a very general application, by no means confined to statistical theory ; and we shall use them . . . for relativistic treatment of the time coordinate or the phase or both together. But Dirac *wave-functions*, which are used to represent probability distributions, are confined to statistical mechanics ; and the common idea, that the fourth coordinate conceived in them is the time, is evidently a misapplication. As shown above, it is the phase that is relativistically connected with the space-coordinates, like the time in non-statistical theory. The invariance of Dirac's wave-equation is not Lorentz-invariance—not invariance for changes of motions of the system or the axes of reference. It is an invariance involving the phase-coordinate, which has formal analogy with Lorentz invariance. We shall find later that the phase is the time-analogue in the analogy between quantum and classical mechanics . . .

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[1] Cambridge, 1946

It should be noticed that in these passages Eddington uses the word ‘ time ’ in such a way as to designate only one of the two time orders we have distinguished ; the one associated with ‘ transitions ’ in the extensive aspect of temporality. This is due to a mere prejudice on his part against the ‘ scarcely imaginable results, e.g. the two-dimensional time’ [1] which he finds naturally associated with his own theory of the geometrical structure of physical space-time, and which he spends much of his work in attempting to explain away. And this prejudice is, I think, attributable, at any rate in part, to the idea that if the ‘ extensive aspect ’ of time is twofold, the ‘ becoming ’ aspect must be also.

It is very important to be quite clear about this distinction between the extensive and becoming aspects of temporality, because the failure to distinguish them has led amongst other things to an altogether unnecessary prejudice against the idea that time may have two dimensions. It has been supposed (for instance by Professor Price) that a two-dimensional theory of time must involve a twofold becoming of events ; that is to say, a ‘ double-now.’ [2] No such idea is implied by the theory I am seeking to put forward. Such a double-now conclusion rests essentially upon a confusion between the ‘ extensive ’ aspects and the ‘ becoming ’ aspects of time. Once the distinction is properly grasped, there is no more reason to think that *two separate* fluxes of becoming must flow independently, because temporality has two extensive aspects, than there is to suppose that any displacement in space must involve *all* spatial dimensions.

It was this philosophical confusion which apparently led Eddington to base his argument for introducing a second variable with a time-like character, in addition to the time-like variable already in use, on the extraordinary proposition we have already quoted ; namely that ‘ “ probability distribution over time ” has no intelligible meaning.’ But Eddington himself uses the words ‘ transition-probability per unit time,’ [3] in connection with his discussion of transition probability by the method of gauge transformations where he introduces the *two* time-like variables ‘ t ’ (appropriate to transitions) and ‘ t_0 ’ (which he calls the ‘ phase ’ appropriate to steady-state wave functions). Unless ‘ probability distribution over time ’ can be said to have some meaning, it is certain that the notion of a mathematical expression, giving ‘ the transition probability *per* unit time ’, can have none either. What, of course, Eddington meant comes out in his remark that ‘ a distribution function $f(x, y, z; t)$ gives the distribution *over* x, y, z, *at* the time “ t ”.’[1] This is a remark with which I agree on the assumption that he meant by it to show that, whenever we prescribe stationary probability distribution functions, which involve a time-like variable ‘ t,’ we are by implication introducing yet another time-like variable. The former variable is of the first time order appropriate to functions in which the phase variable is an argument; and the latter to the second time order, indicating the times ‘ *at which* ’ rather than ‘ *through which* ’ the stationary probability distribution obtains. But, of course, a particular probability distribution which is a function of the phase time variable may cease to hold *at* any subsequent moment of transition-time, the system having undergone what is currently called a ‘ transition.’ And it is clear from Eddington’s discussion in *Fundamental Theory*, pp. 71 ff., that the fully relativistic expression has to be of the form $f(x, y, z, T; t)$, where T represents the phase-variable, not Eddington’s ‘ coefficient of underobservation.’

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[1] Op. cit. p. 126

[2] Vide Price’s paper on ‘The philosophical implications of precognition,’ *Aristotelian Society*, 1937, Supplementary volume **16**, 225 ff.

[3] *Fundamental Theory*, p. 254

I certainly agree with Eddington that the symbol ‘ t,’ occurring as an independent variable in the exponent of Dirac wave functions in statistical mechanics describing stationary states, logically cannot be the same independent variable as occurs in functions describing transitions and unsteady motions. But I fail to see any significance in the reservation implied in Eddington’s calling the first variable only a ‘ time-*analogue* ’ and not a time-like *dimension*. For the analogue must have precisely the same logical structure as the normally accepted time dimension; e.g. its matrix representative is antithetic to the space variables, and it undergoes hyperbolic rather than circular rotations in a Lorentz-like transformation. As indeed Eddington himself remarks: ‘ The scale and phase dimension have therefore a time-like relation to the spatial momenta and coordinates.’[1]

There is according to quantum theory no law which determines with certainty the occurrence of transitions in a single system in a particular environment; or which entails that there must be such and such particular transitions after a certain interval of phase-time, so that the system will certainly pass from the state Ψ_1, to the state Ψ_2. On the contrary, in any given case, the most that can be laid down is that transitions occur according to an assignable probability law. Nonetheless, according to current views, there will be, in the case of macroscopic systems (very large assemblies of micro-constituents) so high a probability as to be a practical certainty that such and such transitions will have occurred during a certain time between a specifiable number of steady states in a large assembly (the number of which can be calculated according to the principles of probability theory) in a specified system in a particular environment of perturbing energies, owing to the enormous number of such states comprised within the macroscopic system. There is, then, a statistical-probability linkage between the (ensemble) average becoming of transition-events in transition-time and intervals of phase-time, in macroscopic structures (such as actual clock-mechanisms), which are in quantum theory represented by very large assemblages of occupants of elementary steady states. But if an attempt were made to investigate the exact relationship between the succession of event-phases, and the occurrence of transitions in particular individual systems, we should encounter the uncertainty or indeterminacy prescribed by Heisenberg's principle in regard to energy and time.

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[1] *Fundamental Theory*, p. 71

The question will now suggest itself to some readers ; granted that there may be an unrecognised duality of time dimensions implicit in certain branches of fundamental physics, e.g. in relativistic quantum theory, is it possible to suggest any operational method of distinguishing them ? In other words, to what type of measurement can transition-time be especially related, and to what type phase-time ? This is an important question over the discussion of which I shall have, I fear, to be too brief for lack of space. It bears an obvious similarity to the one which arises on Milne's dual theory of time scales, and which he puts in the following way : ' The question now arises, can we identify any one of the possible modes of graduation of the clocks of the equivalence with what is ordinarily understood as *physical time ?* '[1]

The answer which Milne gives, on the basis of his investigations of the dynamics of a free particle, is : ' The conclusion from this investigation is that, in Newtonian physics, the independent time variable is *T*-time not *t*-time.' Now our present clocks are calibrated in units standardised in terms of the rotation of the earth relative to the fixed stars. The standard unit of the ' mean solar second ' is calculated on the basis of Newtonian dynamics.[1] Therefore, we may conclude that present-day macroscopic clocks standardised, or regulated mechanically in such a way as to keep their motions steady, deliver a *T-time measure*, ultimately expressible in terms of the unit ' mean solar second.' On Milne's theory this is the natural gauge unit associated with a succession of phases of a steady motion, which is structurally the same kind of motion as the one we have been discussing in connection with the steady circulation ascribed by quantum theory to the stationary electronic states in atomic and molecular systems. We can, therefore, say that the ordinary present-day clocks, calibrated on a Newtonian-mechanically defined standard, measure directly intervals in our phase-time dimension.

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[1] Vide E. A. Milne, ' Gravitation without general relativity,' in *Albert Einstein. Philosopher-Scientist*, ed. P. A. Schilpp, Library of Living Philosophers, VII, Evanston, Ill. 1949, p. 419

Now we come to the other side of the question, viz, to what kind of measurements is the second time variable related? Here again Milne's answer to the analogous question, arising on his dual theory of time scales, is relevant. Milne held that the t-time scale was specially appropriate to, and connected with, the transition-processes within atomic systems; and further, that the electromagnetic frequencies associated with the light-quanta emitted or absorbed in such transitions, given by the Einstein relation $h\nu = E$, must be constant in t-time measure.[2] On the other hand such frequencies would increase secularly, if they were to be measured on the T-scale; though according to Milne's calculations the difference between such measurements on the two scales, which turns upon a parameter determined by the present age of the universe, is so slight as to be inappreciable by direct methods. It can only be estimated by inferences based upon astronomical data from observations of the nebulae. It is clear, however, that, whereas Milne's T-scale is the natural gauge for calibrating clocks measuring phase-time, yet his t-scale is to be regarded as the gauge to be associated with clocks stabilised or regulated by an atomic transition process.

Till quite recently this latter idea would have been a mere intellectual curiosity. But the actual construction of the so-called ' atomic clocks,' which are in fact just mechanisms for using the spectral structure of certain absorption transitions in particular molecules to stabilise the frequency of an alternator used to drive a clock, has lent these ideas considerable practical importance. The frequency stabilised in terms of a molecular or atomic absorption line would certainly be a direct measure of transition-time rather than phase-time; and the appropriate scale would be Milne's *t*-scale. On both Milne's view and mine, therefore, the recent proposal by Doctor R. D. Huntoon and Doctor U. Fano of the U.S. Bureau of Standards,[1] to replace the mean solar standard of time by an atomic standard, based on the (3, 3) line of the absorption spectrum of NH_3 molecules independent of temperature and pressure, would have far-reaching theoretical implications ; though the quantitative difference in terms of these two units in respect of measurements of terrestrial time intervals would be negligible for most practical purposes. And, owing to the extremely minute difference between the mean solar second of phase-time and atomic transition second of transition-time, and the probability linkage we have mentioned above, for most purposes we could continue to ignore the distinction between these two variables, as we undoubtedly do in our daily life.

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[1] Herbert Dingle, *Special Theory of Relativity*, 3rd ed., London, 1950, pp. 37-39

[2] Milne, op. cit. p. 429

But even admitting that relativistic quantum theory does require such a twofold conception of the dimensionality of the extensive aspects of temporal facts in physics ; and that it does demand that we should draw a distinction between physical time-orders analogous to the distinction we have found it necessary to draw in the case of psychological phenomena of the specious present ; yet it will be said there is no real correspondence between these two sets of facts. For, even if we are to assume a complete theory of psycho-neural parallelism, there would still be an enormous difference in the scale of the two kinds of phenomena. Those to which Heisenberg's uncertainty principle is practically relevant are on a very small scale indeed, being in fact submicroscopic; the scale on which, for instance, an uncertainty in the energy of one electron volt involves a conjugate uncertainty in the extensive aspect of the time of less than 10^{-15} of a second ; whereas the phenomena revealed by psychological analysis of the specious present must be macroscopic, since they concern a comparatively large physical entity, the human brain. This would certainly be the view of many scientists today. But there are two considerations which in my view make such an argument invalid. The first point is that some, at any rate, of the receptive organs of sense, with which the human body (viewed as an energy-structure according to the principles of quantum mechanics) is equipped, are amongst the most highly sensitive systems known for the appreciation of very small energy changes. The second point is that Heisenberg's principle prescribes only the lower limit to the range of uncertainty or indeterminacy in simple mechanical inorganic systems, which are largely composed of constituents superposed with phases that are indeterminate in a precisely definable sense. I shall consider these points in turn.

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[1] *Nature*, 1950, **166**, 167-8

First, careful physical, physiological and psychological experiments have shown that parts of the human organism are capable of response to, or appreciation of, physical stimuli which comprise a very small number of quanta of action. We may cite the case of light-quanta falling upon a human eye which give rise to a subjective impression of light. In a recent survey of these phenomena, entitled *Vision and the Eye*,[1] by Dr M. H. Pirenne, we find on page 82 the statement that 'at the threshold of vision the number of quanta actually absorbed by the visual purple in the rods (in the retina of the human eye) is only 5 to 14 quanta. This comes decidedly near to the absolute limit of one quantum.' And he further emphasises : 'As a reactive system, a retinal rod thus reaches the absolute limit of sensitivity set by the quantum and molecular theories.'[2] Further details regarding this aspect of the physical energies involved in the physiology of vision are to be found in an article entitled "Energy, Quanta, and Vision" by Messrs S. Hecht, S. Schlaer, and M. H. Pirenne in the *Journal of General Physiology*.[3]

Secondly, Heisenberg's principle of uncertainty only prescribes a *lower limit* to the indeterminacy introduced by the quantum principle of superposition. This lower limit is the nearest which a quantum-mechanical system can approach to the concept of a completely determinate system as conceived in classical mechanics. In many cases the degree of indeterminacy will be greater : for instance, as Dirac says, it 'holds only in the most favourable case.' It has been shown such minimum uncertainty implies complete independence of the wave functions describing canonically conjugate variables associated with different degrees of freedom obtained as solutions of the Schrödinger equation for a function corresponding to a normal distribution.[4] The necessary condition of complete independence is satisfied by those wave functions used generally in quantum theory to describe what Eddington called a 'natural environment.' Eddington says 'the essential features of a natural environment are represented by a uranoid of particles within incoherent phase.'[5]

[1] London, 1948 [2] Op. cit. p. 83 [3] 1942, **25**, 819–40

[4] Vide for references paper by J. E. Moyal, 'Stochastic processes and statistical physics," *Journal of the Royal Statistical Society*, Series B, 1949, **11**, 185

[5] *Fundamental Theory*, p. 72

But the introduction of any phasic coherence, between the wave functions describing particular particles, or collections of particles, involves a departure from this condition of independence, which is necessary for minimum uncertainty. Such a situation obtains in all molecular structures the stability of which is due in part to the lowering of the total energy through the effects of a quantum mechanical resonance. As has been pointed out : 'this lowering (of the total energy) takes place, however, only if the electronic wave functions corresponding to the different structural formulae (between which the resonance obtains) are superposed with appropriate phases.'[1] Moreover, there are reasons for believing that configurations stabilised to a high degree by resonance energy are characteristic of living, as opposed to inorganic, matter. Nervous tissue is supposed to be, from the quantum mechanical view, one of the most highly resonant forms of living matter ; and the central nervous system of a human brain is probably a supreme instance of a highly resonant structure.

A high degree of quantum resonance implies a high degree of coherence of phase, though the exact degree will depend upon the tightness of coupling. As far as the physics of perception is concerned, the main point here is that the human nervous system in general, and the cerebral cortex in particular, is to be treated as a very complex system in which very many of the constituents are very closely coupled in resonance. And the consequence of this is that we get resultant overall states, which comprise elements drawn from a very large proportion of the whole range of the states, occupied by the ultimate elements, out of which the human brain is built. Of course, this would be a simplification ; and there are probably many processes going on in parts of the brain at any one time which may have little or no relation to the coherently integrated processes in the main structure. But the physiological evidence, particularly that derived recently from the electro-encephalograph, does suggest that a very considerable portion of the brain is a kind of highly unified superstructure that is coherently integrated, largely by relations analogous to resonance-coupling. (Indeed the American physiologist, Lashley, has suggested that such resonance is the essence of memory-functions in man.[2]) It will follow that the spread of compresence must be extended to cover all states which are in any way participant in this resonant superstructure.

[1] Vide F. O. Rice and E. Teller, *The Structure of Matter*, New York, 1949, p. 274

[2] Vide K. S. Lashley, *Brain Mechanisms and Intelligence*, Chicago, 1929, *passim.*

The precise extent to which the basic Heisenberg lower limit of uncertainty or indeterminacy gets enlarged when there is a significant degree of phase-coherence in a structure such as the human nervous system cannot be calculated even approximately with present mathematical resources. Resonance effects can only be roughly estimated at present, even in the simplest possible systems. By a simple system I mean one with only a few elementary constituents, e.g. a few electrons, protons and neutrons subject to small perturbing energies. It is certainly impossible to give a figure which would represent the multiplier required in the case of so vastly complex a structure as the human nervous system, which comprises an immense network of cells, each of which is a most complex system comprising many molecules with a very large number of atoms and a still larger number of elementary constituents. Each such organic molecule may be a highly resonant structure, and there is reason to believe the molecules are themselves bound together in the nuclear structures which probably control the behaviour of cells by a form of resonance energy.

Thus at the moment it is quite impossible to give a precise estimate of the degree of coherence even in a single nerve-cell, let alone in a chain of such cells. But it is possible even now, by this kind of qualitative discussion, to see how the effect of resonance-coherence can prevent the basic Heisenberg modulus of uncertainty of indeterminacy being swamped by the normal process of macroscopic averaging, through the introduction of a systematic bias, leading to cumulative departures from the expectations derivable from distributions applicable to incoherent constituents.

On the view I am putting forward experiential events, such as seeing a flash of lightning, are the inner subjective aspect of processes of prehension, describable in their outer aspects as *physical* events, in which a highly coherent structure, namely the human nervous system, reacts to a disturbance produced by electromagnetic radiation. The human nervous system being a macroscopic structure, the Heisenberg uncertainty or indeterminacy, attaching to the different transitions evoked by a disturbance, would certainly be swamped by the effects of the large numbers involved, according to probability theory, were it not for the cumulative effects of the bias due to coherence.

The upshot of all this may be summarised thus : when we go into the question of the physical counterpart to the relation between event-phases compresent in an experiential event (according to Russell's notion of compresence) ; and, adopting the standpoint of psycho-neural parallelism, look upon the processes in the human brain as the outer physical aspect or counterpart of introspectible processes (which in their inner aspects are psychological); we find that the minimum range of uncertainty or indeterminacy, or compresence (which according to Heisenberg's principle must attach to the temporal relations of energy-transition), may be greatly enlarged. From this standpoint the processes occurring in the brain, which are psycho-neurally parallel to conscious activity on the psychological side, involve transitions between energy levels in the neurones, and their connections, that constitute a very complex and extensive superposition structure of energy levels, coupled together by resonance-coherence in varying degrees, the closeness of the coupling varying directly with the degree of phasic coherence. In this way the physiological mechanism of response to a stimulus, such as a flash of forked lightning, may be regarded as the taking up, or prehension, by a complex machinery of neurones in a brain with highly resonant internal structure, of a certain number of quanta. The energy is originally carried by, or contained in, a disturbance of the environment which impinges upon the periphery of the structure. At the periphery of the nervous system the temporal uncertainty, or span of compresence, judged by reference to some standard clock or time-base of an oscilloscope, might be negligible. But as this disturbance reverberates up the immensely complicated network of the central nervous system, to the relevant cortical area, it is multiplied enormously. The precise multiplication factor cannot be estimated until a much closer estimate (*a*) of the number of cells and elementary constituents involved, and (*b*) of the average degree of coherence obtaining, can be given. But we can see already how in the subjective appreciation or apprehension of disturbances corresponding to processes in the brain, which probably involve other bodily regions as well, a total process, such as seeing a flash of lightning, may well involve temporal indeterminacies, or 'complexes of compresence,' spreading over the kind of time-extent normally characteristic of the human specious present; which is of the order of one second normally. These complexes of compresence would have to be conceived as extended in the two dimensions of time I have indicated in the course of this paper.

H. A. C. Dobbs

SCIENCE WITHOUT PROPERTIES [*]

I

In this paper I wish to show how far it is possible to construct language-systems for scientific purposes in which there is no talk of properties or relations or sets or classes, or, as it is sometimes expressed, language-systems in which no words or other signs are said to designate abstract entities.

I do not propose to spend much time in explaining in detail the motives which have led me to attempt this. They are partly practical and partly theoretical. From what I call the practical point of view, I was led to attempt this because I have found that some of the difficulties which have been encountered in the history of genetics, for example, are traceable to the use of words like ' property ' or ' character ' in such sentences as :

'Characters are transmitted by heredity.'

' Tallness in garden peas is a hereditary character.'

My theoretical motives have arisen from the difficulty I have had in understanding what logicians have written about properties. For example, in J. S. Mill we find :

> We have a name for the objects which produce in us a certain sensation : the word *white*. We have a name for the quality in those objects, to which we ascribe the sensation : the name *whiteness*. But when we speak of the sensation itself . . . language, which adapts itself for the most part only to the common uses of life, has provided us with no single-worded or immediate designation ; we must employ a circumlocution, and say, The sensation of white, or The sensation of whiteness.[1]

I must confess that I do not understand this at all. Coming to quite recent times I find that R. Carnap writes as follows :

> It is important to note what is *not* meant here by the term ' property '. First, it does not refer to linguistic expressions. . . . Second, the properties of things are not meant as something mental, say images or sense-data, but as something physical that the things have, a side or aspect or component or character of the things. If an observer sees that this table is red, then the table has the character Red and the observer

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[*] Received 30. iv. 51

[1] *A System of Logic*, London, 1925, p. 33

> has the corresponding character Red-seeing. By the property Red we mean the first, not the second ; we mean that physical character of the thing which the physicist explains as a certain disposition to selective reflection. . . .[1]

I must confess that I do not understand this either. It may be that my inability to understand these celebrated authors is a purely personal defect which is of no interest to anybody, except perhaps to a psychologist. But I strongly suspect that some people at least will share my difficulties, because I very much doubt whether what is said in these passages agrees with the way in which we use words like ' white ' and ' red ' either in the ordinary or in the scientific use of English. I am even prepared to doubt whether all these statements satisfy the logical empiricist's criterion of meaningfulness. I am inclined rather to believe that they contain some smouldering fragments of metaphysics which have not yet been burnt and purged away in its purifying flames.

The whole notion of ' having properties ' seems to me to be suspect. It may be that the notion of a thing having properties is a vestige of an animistic extension of the notion of a person having property (in the sense of chattels) to things other than persons. Please note that I am not saying that statements containing the word ' property ' are meaningless ; far from it. I am only saying that they are obscure, and I am interested in the problem of devising a linguistic apparatus for scientific purposes which will avoid this obscurity. Some usages of the word are easily avoided although at the cost of much circumlocution. But these are not the difficult ones. For example, when someone speaks of the properties of sulphur he is referring to certain laws or universal general statements concerning sulphur—that it melts at such or such a temperature and so on. With this use of the word I am not concerned. I am only concerned with the word as it occurs in the passages from Mill and Carnap which I have quoted.

But I heartily agree with what Carnap has said in a recent admirable article : ' Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them ; the work in the field will sooner or later lead to the elimination of those forms which have no useful function.' [2]

As regards the notion of set or class, this does not have the objectionable features which are associated with the use of 'property' either in practice or theory, but there are other difficulties. Writers on set theory usually begin by giving examples of sets and then go on to explain that after all these are not examples, because sets are abstract entities of which examples cannot be given.[1] And yet set theory can be successfully used in natural science in spite of this difficulty, and this seems to be a somewhat paradoxical situation.

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[1] *Meaning and Necessity*, New York, 1947, p. 20

[2] ' Empiricism, Semantics, and Ontology ' in *Revue Internationale de Philosophie*, No. 11, Paris, 1950, p. 40

But instead of discussing these problems any further in a general way I propose to take up a challenge which Carnap throws down in the article already mentioned when he says: 'The critics will have to show that it is possible to construct a semantical method which avoids all reference to abstract entities and achieves by simpler means essentially the same results as the other methods.'[2]

How far I have succeeded in meeting this challenge I must leave to your judgment, although, as you will see, I do not claim to have met it completely yet. All I shall offer is a sketch of a method which will provide a basis for discussion.

The statements I am about to make will not belong to the language to be constructed but will tell you about it and will therefore belong to its *metalanguage*. For this purpose I shall use whatever parts of the English language are convenient. In this metalanguage I shall use chiefly the notions of *naming* and of the *concatenation* or horizontal juxtaposition of names and other signs. The chief novelty of the method (if it can be called a novelty) lies in treating names not as names of sets but of the objects which are said to be the members of the sets.[3]

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[1] See, for example, Fraenkel, *Einleitung in die Mengenlehre*, 3rd edition, Berlin, 1928, pp. 4 and 13. See also Quine, *Mathematical Logic*, New York, 1940, p. 120.

[2] R. Carnap, op. cit. p. 40

[3] Thus Hobbes wrote: 'and some (names) are *Common* to many things; as *Man*, *Horse*, *Tree;* every one of which though but one Name, is nevertheless the name of divers particular things; in respect of all which together, it is called an Universall; there being nothing in the world Universall but Names; for the things named, are every one of them Individual and Singular.' *Leviathan*, First Part, Ch. IV. Professor Tarski has also drawn my attention to the fact that the language system proposed here closely resembles one constructed by S. Leśniewski in several respects. My knowledge of Leśniewski's system is confined to what Tarski has told me, or has written, about it (e.g. in the notes to his *Wahrheitsbegriff in den formalizierten Sprachen*, 1935). Leśniewski is an author who makes exceptional demands on the time of his readers. This I discovered when I tried to read his system of ontology. As I had already spent a great deal of time on the study of *Principia Mathematica* I felt that the study of an entirely new and difficult system (one which is not contained within the covers

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1. The language to be described (and referred to here as 'the language WL') contains signs of two kinds: (I) names, and (II) signs of operations. The latter are further divisible into: (*a*) signs of operations upon *names*, and (*b*) signs of operations upon *statements*.

2. With every name in WL is correlated a cardinal number, called the cardinal of the name, namely the number of objects named by that name. Instead of writing 'the cardinal number of the name N is p' we shall write: '$C(N) = p$.'

3. Among the names in WL two occupy extreme positions, namely the name with lowest cardinal (zero), and the name with the greatest (u). The first will be called lambda in the metalanguage and written 'Λ' in WL. It may also be called the *fictitious* name in WL because it names nothing. The second is called Vee in the metalanguage and written 'V' in WL. It may also be called the *universal* name because it names everything about which the statements of WL speak. Thus we can write: C(lambda) = 0 and C(Vee) = u.[1]

4. Names are either simple or composite. Simple names are those having no parts which are names and are not used as abbreviations for names which have parts which are names. Lambda and Vee are simple names in WL. The other simple names are names introduced into WL as undefined signs.

We shall use M, N, and K as metalinguistic variables representing names belonging to WL. For purposes of illustration we shall use 'X,' 'Y' and 'Z' as variables in WL itself. In order to indicate, when occasion arises, that M and 'X' are both functioning as names

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of one volume since it involves back references to other papers, some in Polish) would divert me too much from the biological problems with which I am primarily concerned. Since in my ignorance of Leśniewski I do not appear to be alone, even among those interested in formalised languages, it is much to be desired that some one should make this important and very original system of Leśniewski more widely understood. The principal references are: 'Grundzüge eines neuen Systems der Grundlagen der Mathematik,' *Fundamenta Mathematicae*, 1929, **14**, and 'Über die Grundlagen der Ontologie,' *Comptes Rendus des Séances de la Société des Sciences et des Lettres de Varsovie*, Classe III, 1930, **23**. A continuation of the first of these works, preceded by introductory remarks, was published in *Collectanea Logica*, Warszawa, 1938–39.

[1] A simpler procedure, not involving the general notion of cardinal number, would be to divide all names (other than fictitious and universal names) into: (i) unshared names (those which name only one object) and (ii) shared names (those naming more than one object).

of X we shall write ‘ M = “ X ” ’ and speak of ‘ the object(s) named by M ’ or ‘ the object(s) named by “ X ”.’

5. Composite names are those, in the first place, which are obtained by applying to names operations of the kind (II) (*a*) mentioned in 1.

(i) *Negative names* are constructed by writing a bar over a name. If N is a name in WL then its negative (or Bar N in the metalanguage) will also be a name in WL. Thus if N = ‘ X,’ then Bar N = ‘ $\overline{X}$.’ Bar N names everything which is *not* named by N. Hence if $C(N) = p$ then $C(\text{Bar } N) = u - p$.

(ii) *Cup names* are formed by writing a cup (the sign $\cup$) between *two* names. If N and M are names in WL, then N cup M is a name in WL which names everything which is named by N *or* by M. Thus if N = ‘ X ’ and M = ‘ Y ’ then N cup M = ‘ $X \cup Y$.’

(iii) *Cap Names* are names formed by writing a cap (the sign $\cap$) between two names. If N and M are names in WL, then N cap M is a name in WL which names everything which is named by N *and* by M. Thus if N = ‘ X ’ and M = ‘ Y ’, then N cap M = ‘ $X \cap Y$.’

After these explanations we can formulate the following metalinguistic statements for any names N and M in WL. They all follow from the definitions and the ordinary laws of logic and arithmetic with which we are supposing our metalanguage is equipped :

Theorem 1. $C(N) = C(N \text{cap} \text{Bar} M) + C(N \text{cap} M)$

(for anything named by N will be named also by M or not by M, hence by Bar M).

Theorem 2. $C(N \text{cup} M) = C(N) + C(M) - C(N \text{cap} M)$.

for, by Theorem 1,

$$C(N) + C(M) = C(N \text{cap} \text{Bar} M) + C(M \text{cap} \text{Bar} N) + 2C(N \text{cap} M).$$

Theorem 3. If $C(N \text{cup} M) = C(N \text{cap} M)$

then

$$C(N \text{cap} \text{Bar} M) = C(M \text{cap} \text{Bar} N) = 0 \text{ and } C(N) = C(M).$$

Theorem 4.

If $C(N \text{cup} M) = C(N) + C(M)$ then $C(N \text{cap} M) = 0$.

6. In addition to the above operations on names we also use in WL an operation on names which yields not a new name but a *statement* of WL. This operation has no sign because it is simply the operation of writing two names together in horizontal juxtaposition with minimum spacing between them. In the metalanguage we use

the sign $\frown$ to indicate this operation, which is called concatenation. This sign enables us to construct a name for the corresponding statement. Thus if N = ‘ X ’ and M = ‘ Y ’ then $N^\frown M$ = ‘ XY.’ Concatenation of names with other signs in WL will be indicated in the metalanguage by juxtaposition.

There are *two* signs in WL of the kind mentioned under (II) (*b*) in 1 : the word ‘ not ’ for the operation of *negating* a statement, which is called the negation-sign in the metalanguage (abbreviated to ‘ neg ’); and the sign ‘ & ’ for the operation of *forming the logical product* of two statements in WL, which is called the ampersand in the metalanguage (abbreviated to ‘ amp ’).

After these explanations we can now formulate the following simple *syntactical rules* which specify those combinations of the signs of WL which are to be regarded as *statements* in WL.

Rule 1. If N and M are any names in WL, then $N^\frown M$ is a *simple* statement in WL.

Rule 2. If S is any statement in WL then neg S is a statement in WL.

Rule 3. If S and S′ are any statements in WL then amp SS′ is a statement in WL.

Thus the statements of WL are the simple statements of WL together with all the expressions obtainable by applying to them and to the resulting expressions the operations of negating and of logical product formation any finite number of times. Just as the names of WL are the simple names of WL together with all the expressions resulting from the application to them and to the resulting expressions any finite number of times of the various name-forming operations on names, some of which have not yet been mentioned. Names introduced into WL by definition would be additional to the above.

Next we have the following three simple *semantical rules* which determine those statements of WL that are to be regarded as *true* statements in WL.

Rule 4. A *simple statement* in WL is true if and only if everything named by its first or left-hand name is *also* named by its second or right-hand name.

Rule 5. If S is any statement in WL then neg S is true if and only if S is not true.

Rule 6. If S and S′ are any statements in WL then amp SS′ is true if and only if S and S′ are both true.

By saying that a statement in WL is false we shall mean that it is not true in the sense established by the above rules.

7. We shall say that two names N and M in WL are *synonyms* if and only if *both* of their concatenations are true, and we shall then write: $N = M$. But if at least one concatenation is false we shall write: $N \neq M$.

8. By means of the definitions and rules so far given the following theorems can be proved for any names N, M and K, provided we have in our metalanguage the laws of the calculus of statements:

Theorem 5. If $C(N) = C(M) = 0$ or $C(N) = C(M) = u$, then N and M are synonyms.[1]

(We therefore need only one fictitious and only one universal name in WL.)

Theorem 6. BarBarN = N.
Theorem 7. C(NcapBarN) = 0.
Theorem 8. C(NcupBarN) = u.
Theorem 9. NcupM = McupN.
Theorem 10. NcapM = McapN.
Theorem 11. If $C(N) > C(M)$ then NegN⁀M is true.
Theorem 12. If N $\neq$ Vee then Vee⁀N is false and N⁀Vee is true.
Theorem 13. If N $\neq$ lambda then N⁀lambda is false and lambda⁀N is true.
Theorem 14. N⁀N is true.
Theorem 15. If amp N⁀M M⁀K is true then N⁀K is true.
Theorem 16. If N and M are both not synonymous either with lambda or with Vee, we have one of the following possibilities:

(i) amp N⁀M M⁀N is true, whence N = M,
(ii) amp N⁀M negM⁀N is true, whence N = NcapM.
(iii) amp negN⁀M M⁀N is true, whence M = NcapM.
(iv) amp negN⁀M negM⁀N is true, whence:

(*a*) NcapM $\neq$ lambda and NcupM $\neq$ Vee, or
(*b*) NcapM = lambda and NcupM $\neq$ Vee, or
(*c*) NcapM $\neq$ lambda and NcupM = Vee, or
(*d*) NcapM = lambda and NcupM = Vee.

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[1] Professor Tarski has pointed out to me that Theorem 5 would be false if u is infinite. If WL contains the notion of before in time (see below) introduced by postulates of the kind usually adopted for this purpose, then u would not only not be finite, it would not even be countably infinite.

(If we interpret ‘ B ’ as ‘ the set of all names in WL,’ ‘ $N < M$ ’ as ‘ $N^\frown M$ ’ ‘ $N = M$ ’ in the way explained in the text, ‘ $N + M$ ’ as ‘ N cup M,’ ‘ $N . M$ ’ as ‘ N cap M,’ ‘ O ’ as ‘ lambda,’ ‘ 1 ’ as ‘ Vee ’ and ‘ $\overline{N}$ ’ as ‘ Bar N,’ then we find that Postulates I-VII of Boolean algebra given on p. 509 of Tarski's ‘ Grundzüge des Systemenkalküls I ’[1] are all satisfied. Our metalanguage thus provides an interpretation of Boolean algebra in which the values of the variables are *names* not classes.)

9. We shall now show some connections between WL and the usual language of the theory of sets or classes. For this purpose we must introduce the implication sign into WL, namely the sign $\supset$ which will be called ‘ imp ’ in the metalanguage. If S and S′ are statements of WL, then imp SS′ will be equivalent to : neg amp S neg S′ and will be true except when S is true and S′ is false. If $N =$ ‘ X,’ $M =$ ‘ Y,’ and $K =$ ‘ Z ’ then the correlate in WL of Theorem 15 will be :

$$\supset \& XY\,YZ\,XZ$$ [2]

In the theory of classes there will be the following correlates of this theorem according to the following special cases :

(i)	$C(N) = C(M) = C(K) = 1$	$(x = y \,.\, y = z) \supset x = z$
(ii)	$1 = C(N) < C(M) < C(K)$	$(x \epsilon Y \,.\, Y \subset Z) \supset x \epsilon Z$
(iii)	$1 < C(N) < C(M) < C(K)$	$(X \subset Y \,.\, Y \subset Z) \supset X \subset Z$
(iv)	$1 < C(N) = C(M) = C(K)$	$(X = Y \,.\, Y = Z) \supset X = Z$

10. We next introduce a new operation on names which yield what will be called *bracket names*. In the metalanguage we put *lp* = ‘ (,’ and *rp* = ‘) ’—left and right parentheses respectively.

We shall say that if N is a name in WL and N*lp rp* becomes a name when a name is inserted between the parentheses, then N*lp rp* is a *name-forming functor*. If M is a name in WL we shall call N*lp*M*rp* a bracket name in WL.

We can illustrate the use of bracket names after we have introduced the undefined sign Pee (the letter P) into WL by means of the following rules :

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[1] In *Fundamenta Mathematicae*, Warszawa, 1935, 25

[2] This may be read : if XY and YZ then XZ. In practice it would be clearer to write this :

$$(XY \,\&\, YZ) \supset XZ$$

This is not done here simply in order to avoid the use of brackets and the consequent complication of the rules of statement formation. We owe this device to Łukasiewicz.

Rule 7. Pee is a simple name in WL which names everything which is a *spatio-temporal part* of something.

Rule 8. If N is any name in WL then Pee *lp* N *rp* is a bracket name in WL which names everything which is a part of something named by N.

Rule 9. Converse Pee is a simple name in WL (written '$\breve{P}$' in WL) which names everything of which something is a spatio-temporal part.

Rule 10. If N is any name in WL then converse Pee *lp* N *rp* is a bracket name in WL which names everything of which something named by N is a spatio-temporal part.

In an exactly analogous way we can introduce the undefined sign Tee (the letter T) into WL. E.g.

Rule 11. Tee is a simple name in WL which names everything which is *before* something *in time*.

The rules which are analogous to 8, 9 and 10 need not be given.[1]

With these additions to its vocabulary we can formulate such statements as the following in WL. On the right-hand side I have given the corresponding statement or statements in the language of the theory of relations. In the latter theory Pee and Tee would be said to denote relations (or sets of ordered couples) and would therefore have a different interpretation from that given them in WL by Rules 7-11. The statements in relation theory will take on different forms according to whether the corresponding names in WL name one or more than one object.

(i) XP $x \in D‘P$ or $X \subset D‘P$

This will be true if everything named by 'X' is part of something.

(ii) PY $D‘P \subset Y$

This will be true if everything which is part of something is part of something named by 'Y.' In particular

(iii) PV $D‘P \subset V$

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[1] Professor Tarski has drawn my attention to certain difficulties that might arise from regarding substitutes for relation-signs as names. It might happen that two such signs R and S are such that $R = S$ but $R(X) \neq S(X)$ and the system would cease to be extensional. He suggests two alternatives both of which are used by Leśniewski. One of these resembles mine except that R is not regarded as a name with an independent meaning (like Pee and Tee) but only as an operation sign with which name-forming functors '$R(\quad)$' are constructed. With this interpretation Rules 7 and 11 would no longer be needed.

is true by virtue of Theorem 12. (It is L-true in Carnap's terminology.)

(iv) XP(Y) xPy or $X \subset sg\text{‘}P\text{‘}y$ or $X \subset P\text{“}Y$

This will be true if everything named by ' X ' is part of something named by ' Y.' In particular if ' X ' and ' Y ' each name only one object (iv) will be true if X is part of Y.

(v) P(Y)X $sg\text{‘}P\text{‘}y \subset X$ or $P\text{“}Y \subset X$

will be true if everything which is part of something named by ' Y ' is also named by ' X.'

Owing to the fact that Pee and Tee in WL are not relation-names the false statement (in the language of the theory of relations) that P is included in T, cannot be formulated in WL with the help of Pee and Tee alone by simply concatenating them. Thus the correlate in relation language of Pee$\frown$Tee would not be the statement that P is included in T, but the statement that the domain of P is included in the domain of T. To express the inclusion statement in WL we must write :

$$\supset XP(Y)\ XT(Y)$$

This means that, although relational statements can be formulated in WL, they cannot all be formulated in forms which have exact analogues in the *calculus* of relations, but only in the lower functional calculus for two-place predicates.

We cannot have relation-names in WL for the following reason. The pair of towns contemplated when we say : ' York is north of London ' is the same pair of towns as is involved when we say : ' London is bigger than York.' Therefore I cannot say that ' north of ' names the pair York, London and not the pair London, York, because these pairs are identical. In order to have genuine relation-names I must have *ordered* pairs and these are abstract entities. The distinction between the ordered pair York, London and the ordered pair London, York, lies not in the towns themselves but in the order in which their names are written down, to enable us to express the difference between York being north of London (and not vice versa) and London being bigger than York (and not vice versa).

11. It is assumed (in the present context) that everything named by Vee has a beginning and an end in time. If the beginning and end of anything do not coincide we shall call that thing a *time-stretch*. If the beginning and end coincide we shall call it a *time-slice*. We therefore introduce two new simple names into WL : S*t* which names

every time-stretch, and S*l* which names every time-slice.[1] With these new names we can construct name-forming functors. For example, S*t*(Y) can be explained as naming everything named by S*t* which is part of something named by Y.

12. *Sigma names.* These are names constructed in the following way by means of the Greek letter Σ which is not itself regarded as a name but as an operation sign.

Rule 12. If N is any name in WL then Sigma N is a name in WL which names *the* object z such that everything named by N is a spatio-temporal part of z and every such part of z has parts in common with something named by N ; z is thus the object composed of the objects named by N and nothing else.

13. We now have the necessary equipment for discussing the peculiarities of *proper names.* In ordinary discourse these are ambiguous in the sense of vague, although this does not lead to inconvenience in ordinary contexts. It is only when we try to use proper names in scientific discourse that their ambiguity reveals itself. Proper names are commonly assumed to name one and only one object and thus to be of cardinal 1, and yet they are frequently used as though this was not the case. For example, let us introduce ' W ' into WL as an abbreviation for the name ' Winston Spencer Churchill,' let us use ' L ' as an abbreviation for the name ' member of the Liberal party during 1909 ' and ' C ' as an abbreviation for the name ' member of the Conservative party during 1929.' With these names we can formulate the following statements in WL :

(i) not $\mathrm{W}{\frown}\mathrm{L}\mathit{\Lambda}$; (ii) not $\mathrm{W}{\frown}\mathrm{C}\mathit{\Lambda}$; (iii) $\mathrm{L}{\frown}\mathrm{C}\mathit{\Lambda}$.

The first states that something is named by both ' W ' and ' L ' the second that something is named by both ' W ' and ' C ' and the third that nothing is named by both ' L ' and ' C ' (i) and (ii) express what is commonly accepted as historical fact. (iii) is urged for the following reason : an object named by ' L ' cannot be identical with one named by ' C ' because the beginning and end of the former must be before the beginning and end of the latter in time. But if x is to be identical with y (in the sense in which Paris is identical with the capital of France) its beginning must be identical with the beginning of y and its end must be identical with the end of y. Accordingly ' Winston Spencer Churchill ' names at least two distinct objects and is therefore not of cardinal 1. In WL proper names are treated in this way as names having a cardinal greater than 1.

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[1] These need not be introduced as undefined signs because they can be defined within WL by means of Pee and Tee ; see my *Technique of Theory Construction*, Chicago, 1939. But on the present occasion it is simpler to treat them as undefined signs.

Suppose we introduce into WL the astronomical proper name ' Venus,' together with the expressions ' the evening star ' and ' the morning star ' all as simple names, but using ' V*e* ' ' E ' and ' M ' respectively as abbreviations for them. With these additions to our vocabulary we can formulate the following statements :

$$\mathrm{EV}e, \quad \mathrm{MV}e, \quad \mathrm{E}\overline{\mathrm{M}}, \mathrm{M}\overline{\mathrm{E}}, \quad \text{and } \mathrm{E} \frown \mathrm{M}\ \Lambda$$

The difficulties which have been discussed by logicians in connection with these expressions do not here arise.

In the language WL, such descriptions as ' the author of *Waverley* ' will be names of cardinal 1. Thus suppose ' A ' names every time-stretch named by ' Walter Scott ' during which the writing of *Waverley* was actually going on, A will not be of cardinal 1 because there were, presumably, pauses for meals, sleep and exercise. No one of these time-stretches wrote *Waverley*, but each wrote only a part of it. We can, however, say that 'ΣA ' is a name of cardinal 1 which names the object composed of all the time-stretches which wrote some part of *Waverley*, so that we can write :

$$\Sigma\mathrm{A} = \text{the author of } \textit{Waverley}.$$

It could, of course, be argued that the pauses between the time-stretches named by A also had a share in the writing of *Waverley*, since without them *Waverley* could hardly have been written. It could similarly be argued that the time-stretch whose beginning is commonly referred to as ' the moment of birth of Walter Scott,' and whose end is the beginning of the earliest time-stretch named by A, also had an indirect share in the writing of *Waverley*. Such questions can easily be decided by setting up conventions regarding what we are to understand by the phrase ' the writing of *Waverley*.' But when we now consider the time-stretch which began when the last stretch named by A ended and which itself ended with the moment commonly referred to as ' the moment of the death of Walter Scott,' by no stretch of language can we say that *this* time-stretch wrote *Waverley*. But *all* the time-stretches we have mentioned are named by ' Walter Scott.' Thus ' Walter Scott ' names very many time-stretches, some of which had no share in the writing of *Waverley*. This name is of cardinal greater than 1 and so we cannot write :

Walter Scott = the author of *Waverley*.

Consequently we cannot substitute 'Walter Scott' for 'the author of *Waverley*' in the statement : 'George IV wanted to know whether Walter Scott was the author of *Waverley*.' In WL no antinomies arise in such connections.[1] They arise from the failure to recognise the peculiar ambiguity of proper names and from applying the scientific operation of substitution in a non-scientific context. Even if we say that 'Walter Scott' names only the single time-stretch which began with the birth (or conception) of Walter Scott and ended with his death, still this clearly cannot be identified with ΣA. It did not write *Waverley*, only a part of it did so. And it may well be doubted whether any of Walter Scott's friends used his name exclusively in that sense during his life time. On the contrary it seems clear that this name can be applied to every time-stretch and every time-slice which is a temporal part of this total time-stretch.

One of the commonest ways of obtaining a name of cardinal 1 from a proper name (even in ordinary discourse) is by coupling it with one or two time-coordinates—with one in the case of a time-slice (e.g. Walter Scott at midnight on his 21st birthday), with two in the case of a time-stretch (e.g. Walter Scott from midnight on his 21st to midnight on his 22nd birthday). The very fact that recourse is had to such devices is an acknowledgment that proper names are not of cardinal 1. Among names of cardinal greater than 1, proper names seem to be the only names which thus yield names of cardinal 1 when coupled with time-coordinates. Thus 'man at time t' is still of cardinal greater than 1. The distinctions here involved can easily be formulated by means of Pee and Tee.

14. In WL we thus have a language which, when its vocabulary is sufficiently enlarged by the introduction, as undefined signs, of suitable names according to the subject-matter to which it is to be applied, enables us to express what we want to say in a very precise form, but without it being necessary to say that these names name properties, sets, or classes. It is as powerful as the lower functional calculus for n-place predicates, although we have only given examples corresponding to 1- and 2-place predicates. But values of n greater than 2 can easily be provided for. For example, B(X(Y)) can be

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[1] See R. Carnap, *Meaning and Necessity*, p. 134

introduced as a name which names everything which is *between* something named by X and something named by Y. It is formed from a name-forming functor with two empty places instead of only one.

It will be noticed that WL has the remarkable peculiarity of being a language in which there is no distinction between subject and predicate, although so-called ' subject-predicate propositions ' can be formulated in it. From a purely grammatical point of view WL, in addition to the signs of operations, only contains *nouns*, but no verbs or adjectives.

If, in addition to introducing suitable undefined signs, we also introduce axioms which determine their use, and if we also equip WL with the usual axioms of logic governing the operations of negating and of the formation of the logical product of two statements, and if, finally, we formulate in the metalanguage suitable rules of inference, then WL becomes a deductive system.

15. In conclusion, two problems may be mentioned which have not yet been solved. First, there is the problem of how to introduce into WL names which would ordinarily be said to name, not sets of individuals, but sets of sets of individuals. Thus if we regard ' *Homo sapiens* ' as a name which names all men, how are we to treat such words as ' species ' ? We could say in the metalanguage that ' *Homo sapiens* ' is a species-name, and perhaps we could construct correlates in WL of such metalinguistic statements by creating a new kind of semantical category distinct from names.[1] Or, it is possible that this problem might be solved by the use of sigma names. Supposing this problem to be satisfactorily solved, it does not seem likely that further difficulties of this kind will arise, at least in biology, because an appeal to still higher logical types does not seem to be necessary.

The second unsolved problem is that of the best way of introducing mathematics into WL. This is a problem for experts.

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1. So much for the formal description of the language WL. I propose to turn now to the problems presented by words like ' gene,'

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[1] Where such signs can be introduced into WL by definition there is no difficulty and I then call them *abbreviators*. The problems connected with the quantification of variables representing such signs have not yet been studied. Their use will of course involve additions to Rules 1-6.

'atom,' 'electron,' 'energy,' etc., which occur in the theoretical statements of biology and physics. I propose to call these words names also and, to distinguish them from other names, I shall call them *theoretical* names.[1]

The circumstances under which our natural languages, such as English, have developed have been comparatively simple and primitive. Under these circumstances certain simple linguistic forms have become established. But the circumstances under which these natural languages continue to be used at the present day are very far from primitive and simple. Nevertheless, we still use those same simple linguistic forms amid all the complexities of modern life. It is hardly a matter for wonder that a too naive use of such linguistic forms should sometimes lead us into difficulties. What is truly remarkable is that under modern conditions they have continued to meet the demands put upon them so successfully.

Consider, for example, the use of language in connection with a process to which human beings seem to be particularly addicted: the process of making war. When our language was beginning to take shape men killed each other with clubs and stones. By the time the principal linguistic forms were well established they killed each other with swords, lances and arrows. Then came gunpowder, followed by T.N.T. and other high explosives and now (in conformity with the spirit of modern times) we can do the job on a truly wholesale scale by means of atomic fission.

Now the transition from clubs to swords and from swords to gunpowder was not accompanied by any great linguistic development. But in the transition from gunpowder to T.N.T. and from the latter to atomic fission there does *appear* to have been a very great elaboration of linguistic forms: mathematical and chemical equations, for example, at once come to mind; and there will be few people, I imagine, who will wish to deny that these devices played a great part in the invention of T.N.T. and atomic fission.

But may not this linguistic elaboration be in *some* sense only apparent? About chemical equations I cannot speak because, as far as I know, no one has submitted them to a syntactical and semantical analysis. But as far as mathematics is concerned such analysis has now been going on from the time of Frege at least, and, according to one recent authority,[1] all the linguistic apparatus of mathematics can be constructed with the help of ' neither *p* nor *q* ' ' for all *x* ' and ' $x \epsilon X$ ' ; that is to say with the sentential operators, quantification and set-membership. But set-membership brings us back to statements of the form ' *x* is a club,' ' *x* is a rifle,' ' *x* is an atomic bomb,' etc., which is a form characteristic of the simplest use of language. If this is correct then the elaboration of linguistic forms has not involved anything essentially new, but has consisted in the construction of very elaborate devices out of elements essentially old.

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[1] These are to be introduced into WL by means of postulates, or by means of definitions in terms of other theoretical names previously introduced by postulates as explained below.

Now originally statements of the form ' *x* is a so-and-so ' came into use in situations in which human beings dealt with objects which they could handle. If anyone is in doubt about what we are referring to when we say ' this is a club ' or ' this is a sword,' we can soon satisfy him by putting a club or a sword into his hands. And, as Sancho Panza would say, a bird in the hand is worth two in the bush. Or, we may put it this way : naming originally came into use in connection with situations where it was possible to stick labels on to things.

But this same form ' *x* is a so-and-so ' is also used when we write : ' *x* is a molecule of T.N.T.' or ' *x* is an atom.' But now we only have the bird in the bush and the same method of satisfying a questioner is no longer available, so the problem arises : How in this case are statements of this basic form functioning in our language ?

Leaving this question of the extension of the use of basic linguistic forms on one side for the present, let us return to the question of the significance of the elaboration of complicated forms out of the old ones, still in connection with the invention of new methods of killing. Why has there been this parallel elaboration of language ? In a sense, in manufacturing these new lethal devices we have not departed from the world of bows and arrows. Rifles and bombs are still things which we can handle. But in their construction, and in the manufacture of their accessories, more elaborate tools and apparatus are required than was the case with more primitive weapons. The invention of new apparatus has been one factor in the production of a more elaborate vocabulary, even although this apparatus is made from materials many of which had designations in the older vocabulary.

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[1] W. V. Quine, *Mathematical Logic*, pp. 125–126 : ' We shall see in subsequent sections that the notions of identity, relation, number, function, sum, product, power, limit, derivative, etc., are all definable in terms of our three primitive notational devices : membership, joint denial, and quantification with its variables. . . . The three primitives thus provide a complete mathematical language.'

But the chief and most important factor in the elaboration of language has not been the mere enlargement of vocabulary, but the progressive uniting of seemingly unrelated spheres of discourse. Discourse about breathing and discourse about the rusting of iron and about the burning of candles were brought within a single system by the introduction of the notion of oxidation, and the formulation of hypotheses containing it, from which statements about breathing and statements about rusting and burning could all alike be derived as logical consequences.

Now it has been known from the time of Aristotle that the relation of logical consequence between statements depends not on the content of those statements but on their form.[1] Moreover the syntactical and semantical analysis of mathematics has shown that its statements are concerned only with uninterpreted formulas and their truth-preserving transformations. From the point of view of empirical science, therefore, all the elaboration of mathematics referred to above has been concerned with the process of deriving statements of lesser generality from statements of greater generality, and it is in the statements of greater generality that theoretical names like ‘ gene,’ ‘ atom,’ etc., occur.

If we carry out the laborious task of axiomatising a theory in which we are interested we find that theoretical names occur (for the most part) in the axioms or postulates as undefined signs. . . . If we ask how they got there axiomatisation will not tell us. If we wish to find the answer we must undertake another laborious task which I call methodological analysis and which, in a sense, is the reverse of axiomatisation. In the process of axiomatisation the raw materials of our theory are, for the most part, already at hand and our task is to discover which statements can best be regarded as axioms or postulates from which all the remaining ones can be derived as their logical consequences, by the successive application of the rules of inference of the system, and of the process of substituting real names for dummy ones in formulas expressing truth-preserving transformations which are included in the system. There is also a parallel process of discovering which signs in the system can best be regarded as primitive or undefined signs with the help of which all the remaining signs can be defined. Axiomatisation thus enables us to study a theory in its most compact and concentrated form and to see how all its parts are related.

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[1] For discussions of the difficulties connected with making this distinction precise see : A. Tarski, ‘ Über den Begriff der logischen Folgerung,’ *Actes du Congrès internationale de Philosophie Scientifique*, Paris, 1936, *fasc.* 7 ; K. R. Popper, ‘ Logic without Assumptions,’ *Proc. Arist. Soc.*, London, 1947, pp. 251-292. G. H. von Wright, *Form and Content in Logic* (inaugural lecture), Cambridge, 1949.

But when we undertake what I call methodological analysis the situation is quite different. We are then interested in the way in which the theory has been gradually built up in the first instance, or in possible alternative ways of building it. We therefore start with records of observations in which the names all belong, so to speak, to the bow-and-arrow level and to which therefore the primitive criteria of meaningfulness are applicable. The first step in theory construction is the generalisation of such records—the passage from some to all. Such universal general statements I call *theoretical statements of zero-level.* They are theoretical in the sense that they say more than is asserted by the conjunction of the observation records. The next step is to find some means of linking together a number of such zero-level statements by devising statements from which they can all be derived as logical consequences. In this way we reach first-level theoretical statements. Then the same process may be repeated with these, bringing us to second-level theoretical statements and so on.[1]

Now, sooner or later, as this process is continued, we are compelled to introduce theoretical names, because the requisite link is not provided by the vocabulary which is appropriate to the observational level. If we now reverse the process and undertake axiomatisation we naturally find that these theoretical names are playing the part of undefined signs. They cannot, as far as I can see, be defined operationally. They are introduced by means of postulates. If we are asked what they *mean*, or what they *name*, or what the statements containing them are *about*, all we can reply is, first, that we have no means of applying the primitive criteria and that therefore these questions are not themselves meaningful on the *old* basis ; and second, we can point out that nevertheless from theoretical statements containing these names, zero-level statements belonging to otherwise unrelated spheres of discourse can be derived (through a succession of consequence-steps) and in this way such theoretical statements can be indirectly subjected to the test of confrontation with the results of observation. But the great merit of theoretical statements of this kind lies in the fact that they suggest new ways of putting things together and constructing new devices—hence the invention of new apparatus, including new apparatus for observation—on the bow-and-arrow level. Without the pyramid of theoretical statements the astonishing achievements of technology would, for the most part, have been impossible, because no one would have thought of them. Without the doctrine of molecular structure (or something equivalent to it) there would have been no clue to the synthesis of the many marvellous materials which applied chemistry has made available.

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[1] What is achieved by axiomatisation and methodological analysis can only properly be grasped by the consideration of examples worked out in detail, and such examples cannot be given in a short article. A biological example of axiomatisation is given in my *Technique of Theory Construction*, Chicago, 1939. An example of methodological analysis was given in my Tarner lectures which were delivered at Cambridge in Michaelmas term of 1949, but owing to delays in publication, this example is not yet available.

The foregoing remarks are intended to suggest that we should not ask that the rules which are applicable at the bow-and-arrow stage should be applicable at *all* levels of the scientific development of language. If we insist upon demanding this we shall find ourselves involved in interminable epistemological and metaphysical disputes. I very much agree with Quine when he says : ' The remedy here is simply to give up the unwarranted notion that talking sense always necessitates there being things talked about.'[1] The criterion of talking sense which we actually apply is the success of our language-system in enabling us to continue the process of systematising scientific statements into theories of wider and wider scope and through this in enabling us to do and to make the things we want.

There are, however, two other factors which complicate the issue, namely, visual imagery and faith.

Visual imagery and spatial diagrams have played a very large part in suggesting, and in aiding the construction of, scientific hypotheses. A dot on a piece of paper can represent anything from a planet to an atom, and yet, valuable as such devices are, both heuristically and pedagogically, they play no part in the deriving of consequences from the postulates of a scientific theory when once the latter have been formulated ; just as abstract geometry is independent of the drawings of triangles and circles which we see in school books on Euclid. In genetics, diagrams still play a large part, because the linguistic apparatus which would enable the diagrams to be dispensed with has not yet been fully elaborated. But diagrams, from their very nature, can be an obstacle to generalisation (and have been in genetics), so that what begins by being a help may end by being a hindrance.

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[1] W. V. Quine, *Methods of Logic*, New York, 1950, p. 201. The remark quoted is made in reference to fictitious names.

There may yet be a great future in store for the elaboration of purely abstract scientific theories which function without the aid of diagrams or visual imagery. The art of constructing such theories does not appear to have been cultivated outside modern physics, but, apart from human frailty, there seem to be no insuperable obstacles to this being done. The absence of special mathematical techniques, apart from those devised to meet the requirements of physics, is no doubt an obstacle. But this can, I believe, be overcome to a large extent by the use of elementary set theory, for the use of which there is, I believe, a great future in biology.

So much for visual imagery and diagrams, now a few words about faith. Successful scientific theories generate intense belief; not belief in the language system to which such theories belong, but belief in a world of objects behind the scenes about which the postulates of the theory appear to speak.[1] This belief is aided by the successful use of spatial diagrams, and abetted by notions of meaning which are appropriate to the bow-and-arrow level. But belief belongs to personal biography and, although it too may be important heuristically, it does not enter the scientific theory as such. A scientific theory is successful by virtue of its structure and the non-contradiction of its zero-level statements by observation records, and it is accepted because it is successful, not because people believe it. (Although presumably non-experts accept the theories of experts because the experts believe them.) But belief in a theory may be an obstacle to its rejection when an alternative and better one is proposed. This is abundantly illustrated by the history of science. Therefore, like spatial diagrams, belief may begin by being a help and end by being a hindrance. Because readiness to construct and consider new alternative hypotheses and to abandon old ones has played an important part in the development of science.

Some of the foregoing remarks about theoretical names apply also to words like ‘ property,’ ‘ class,’ or ‘ set.’ These words (especially ‘ set ’) are useful in constructing theories provided we use them rightly (as prescribed by a suitable system of postulates) and do not allow ourselves to become entangled in too naive questions concerning ‘ meaning ’ and ‘ existence ’ in connection with them. If a beginner in set theory asks what a set is we cannot tell him, but as he continues to study the theory he gradually learns to understand its use and that is what matters. Such words are what Carnap, in his *Logical Syntax of Language*[1] calls *universal words.* On page 294 he writes :

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[1] Carnap, on p. 23 of the article already quoted, writes : ‘ To accept the thing world means nothing more than to accept a certain form of language, in other words, to accept rules for forming statements and for testing, accepting, or rejecting them. Thus the acceptance of the thing language leads, on the basis of observations made, also to the acceptance, belief, and assertion of certain statements. But the thesis of the reality of the thing world cannot be among these statements, because it cannot be formulated in the thing language, or, it seems, in any other theoretical language.’

> We all use such universal words in our writings in almost every sentence, especially in the logic of science. That the use of these words is necessary is, however, only due to the deficiencies of the word-language, i.e. to their inadequate syntactical structure. Every language can be transformed in such a way that universal words no longer occur in it, and this without any sacrifice of expressiveness or conciseness.

But I do not wish to suggest that there are no important differences between theoretical names and alleged names of abstract entities. An important difference between them can I think be roughly indicated (without going into details) in the following way. In connection with theoretical names there is a process commonly called *demonstrating* the objects they name by means of more or less complicated *apparatus.* Thus cells can be demonstrated by a suitable microscope, suitably adjusted. Ultra-microscopical objects can be demonstrated by means of an electron microscope and electrons themselves by corresponding complicated apparatus. Although the apparatus used and what is actually *observed* all belong to the bow-and-arrow level and what is *demonstrated* is named by a theoretical name and does not belong to the bow-and-arrow level, yet the observation in the situation provided by the apparatus is *called* demonstrating the object named by the theoretical name. Now in the case of abstract entities there appears to be nothing like this at all : they cannot be demonstrated and examples cannot be given. But this does not preclude the successful use of a calculus in which some of the signs are said to name abstract entities.

2. In order to make the very general remarks of the preceding paragraph more concrete and convincing it would be necessary to show you an actual example of methodological analysis. But to do this is quite impossible within the boundaries of a single lecture. Let me say a little, however, about the results of applying such an analysis to elementary genetics. This example is particularly

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[1] London, 1937

unfavourable to my general thesis because it contains instances of all the difficulties which were mentioned at the end of Section 2. For that reason it is all the more interesting.

Such an analysis shows that given (i) the notion of phenotype ; (ii) a designation for the offspring from a specified type of mating in a specified environment (and thus a name-forming functor) ; and (iii) certain natural and positive rational number-designations, a great deal of zero-level genetics can be precisely expressed. Certain genetical systems can be defined and classified and laws concerning them can be formulated. At this level, and indeed at higher levels when they are reached, the word ' phenotype ' functions as an undefined sign. It is easy to specify particular phenotypes. For example, in set-language we can say that the set of all garden peas with yellow cotyledons is a phenotype, and this way of dealing with the problem would make phenotype itself a set of sets. Alternatively, we can say : ' garden-pea with yellow cotyledons ' is a name of cardinal greater than 1, and is a phenotype name. But we cannot treat the word ' phenotype ' itself as an abbreviator because, at present at least, we possess neither a definition nor a set of postulates for it. Nevertheless, given this notion and the others mentioned above it is possible to formulate a great deal of zero-level genetics.

At the same time those zero-level statements which express general laws concerning phenotypes, since they do not contain words like ' garden-pea ' and ' yellow cotyledon,' do not enable us to mention a single *example* of the laws they express. The question may therefore be raised : how can statements containing such words be derived from theoretical statements of this kind, as they must be if the latter are to be testable ? The answer is that there are two sub-strata among genetical zero-level statements : (i) the immediate generalisations of breeding records which therefore contain designations of particular phenotypes, and (ii) further generalisations of these which speak of types of genetical systems but involve no reference to particular phenotypes. It suffices if such words as ' garden-pea ' and ' yellow cotyledon ' only occur in observation records and in immediate generalisations thereof. Because, given a classification of genetical systems and the laws concerning them, it suffices to supplement these with a catalogue of *instances* of such systems, and it is only in this catalogue that words of the type mentioned need occur. Given the general laws and the statements of the catalogue of instances, other statements about these instances are derivable, and it is by confronting these other consequences with the results of observations that the general laws are tested.

3. I must not leave these topics without saying something about the place of words like ' yellow ' in the language WL. Statements like ' this cotyledon is yellow ' are used for testing scientific theories, and for this purpose it is usually quite unnecessary to worry much about their further interpretation or analysis. This is fortunate because when we do attempt to probe further we stir up a veritable hornets' nest of complications about which there is scope for endless disputes. But I should like to add a few remarks in explanation of my statement that such words as ' yellow ' are names in the language WL, of cardinal greater than 1.

My starting point, in an endeavour to find a tolerably non-committal and therefore, I hope, non-controversial, way of speaking about such matters, is first to invite consideration of such statements as the following :

(i) You get a fine view of the sea from this window ;
(ii) If you listen carefully you get a sound of running water ;
(iii) When I stroke this I get a feel of velvet ;
(iv) You get a lovely smell of lavender in this garden ;
(v) When I swallowed it I got a taste of vinegar.

These statements affirm something about the views, sounds, feels, smells or tastes you *get* under certain *circumstances*. Moreover these things that *you* get are all *of* something—the view is of the sea, the sound is of running water, the feel is of velvet and so on. Is it not clear that these statements are all built upon a matrix of the form :

x gets y which is of z under circumstance w.

In the language WL this has the form of a statement containing a name-forming functor with three empty places. We could say : ' $G(y(z(w)))$ ' names everybody who gets something named by ' y ' which is of something named by ' z ' under circumstance named by ' w. ' By referring to the first, second and third places in this functor and to the objects named by it when these places are occupied by names, we obtain an entirely innocuous terminology for speaking about the objects concerned, which enables us to avoid treading upon people's ontological and epistemological corns.

From this point of view such a statement as ' this table is red ' (to take Carnap's example quoted at the beginning) surely involves a considerable over-simplification, although it serves our daily purposes well enough on the whole. In the language WL it would be interpreted as saying something about the views of the table which the *normal* person gets under *ordinary* circumstances. In the language WL ' red ' is thus treated as naming certain objects whose names can occupy the first blank in the above functor. The correlate in the metalanguage of WL of the statement ' this table is red ' would be roughly somewhat as follows : ' " table " names this and " red " names those views which are *of* this and which the normal person gets under ordinary circumstances.'

In conclusion let me summarise what I have been trying to say : I believe I have shown that it is possible to construct an extremely simple, but at the same time powerful, language, at least for many biological purposes, in which the descriptive signs are regarded simply as names of time-stretches or time-slices of objects, but not as names of properties or sets. In doing this it has been necessary to extend the primitive notion of naming. We admitted fictitious names which name nothing, we admitted variables or dummy names which also name nothing, and we admitted theoretical names which name we know not what. With the help of these names and certain signs of operations it has been possible to construct a language having very simple syntactical and semantical rules, but nevertheless very considerable powers of expression. At the same time I pointed out some important ways in which at present such a language system would be defective. Finally I suggested that the criteria of talking sense in a scientific language are not to be sought in any general extension or application of primitive criteria of meaning, but in the purely pragmatic criterion of the successes of our language in enabling us to formulate scientific theories and through them to do things—to attain certain desired ends.[1]

J. H. WOODGER

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[1] In preparing this paper I have benefited much from discussions with Lord Russell and with Professors Ayer, R. Martin, K. Popper and A. Tarski.

SCIENCE AND LOGIC: SOME THOUGHTS ON NEWTON'S SECOND LAW OF MOTION IN CLASSICAL MECHANICS *

I

FROM time to time it may be of value to engage in an analysis of the logical status of the concepts and laws of physics which are in current use and to investigate their logical and empirical foundations. In discussions of the foundations of mechanics and physics in general, Newton's Laws of Motion have a fairly prominent place. Though much attention has been paid to the First Law an investigation of the Second Law is not without interest, involving as it does a consideration of the related notions of force and mass. From our point of view, however, its chief interest lies in the fact that it illustrates a discussion of certain views concerning the basis of such laws which have been developed during the last seventy years. Two fairly well defined rival positions may be distinguished though there are of course plenty of intermediate ones. A review and criticism of these will help us to appreciate the kind of problem surrounding the relationship that holds between physical laws and their evidence. The didactic purpose of such a review will not be overlooked, and the discussion will proceed at an elementary level. Furthermore, the vectorial character of force will not be considered nor will Relativity be taken into account. We shall try to remain as closely as possible to simple experience.

A fairly accurate translation of the Second Law of Motion as it occurs in Newton's *Principia* [1] is as follows: 'The change of motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.' In modern terminology this may be rendered: 'Rate of change of momentum is proportional to the unbalanced force and is in the direction of that force.' Choosing the usual units and ignoring Relativity effects the law simply becomes 'force = mass × acceleration.'

Of the two main contrasting views affecting the logical status of this law the first holds that the law is an empirical generalisation about the

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* Received 27. ii. 51

[1] F. Cajori, *Sir Isaac Newton's Mathematical Principles*, Cambridge, 1934, p. 13

result of a number of experimental situations involving well-defined relations between forces, masses and accelerations. In other words, there are definite empirical tests which would enable us to confirm or disconfirm the law. This view we shall call the synthetic or empirical view. The alternative doctrine maintains that the Second Law is not an empirical law but functions either as a definition (or contributory definition) of mass and/or force, or as an analytic statement prescribing the way in which motion is to be analysed. In the latter case such a law is sometimes spoken of as a convention or as a functionally analytic proposition. This view we shall call the analytic view. The former was held by Newton and has been defended in some detail by C. D. Broad.[1] It is taken for granted in many traditional textbooks where elementary experiments are mentioned purporting to serve as tests for the law. The analytic view was held by Kirchhoff, made prominent by Poincaré[2] and has been discussed among others by Lindsay and Margenau[3] and by A. Pap.[4] It is not always clear to which camp a writer belongs. Thus Maxwell[5] wrote: 'External or "impressed" force considered with reference to its effect—namely, the alteration of motions of bodies is completely defined and described in Newton's three laws of motion.' He then proceeds to a description of a series of empirical tests.

Space forbids a thorough discussion of all the arguments. It will be sufficient to mention a few of them where relevant and we shall not attempt to demonstrate the 'correctness' of any one exclusively. Our method will rather be to make brief references to the reasons that have seemed to lead to a conclusion favouring one or the other side, to point out the similarities and differences between them, and to suggest some criticisms so as to prevent too hasty an adherence to any particular view. Such criticism is aimed at showing how either view depends upon a selective stress on certain features of scientific method to the exclusion of some others, and it is incidentally designed to prevent the deduction of certain metaphysical beliefs from the procedure of science.

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[1] C. D. Broad, *Scientific Thought*, London, 1923, Ch. 5

[2] H. Poincaré, *The Foundations of Science : Science and Hypothesis*, Lancaster, Pa., 1946, Ch. 6

[3] R. B. Lindsay and H. Margenau, *Foundations of Physics*, New York, 1936, Ch. 3, sects. 3.4–3.6

[4] A. Pap, *The A Priori in Physical Theory*, New York, 1946, p. 48 ff.

[5] J. Clerk Maxwell, *Matter and Motion*, London, 1920, Ch. 3, sect. 40, p. 27

2

There are certain definite reasons for assuming that Newton held the synthetic view. The *locus classicus* occurs in his *Opticks*:[1] '. . . *to derive* two or three general principles of motion *from phenomena*, . . . would be a very great step in Philosophy. . . .' There also, after telling us that the method of physics ought to consist in an analysis and subsequent synthesis, he continues: '. . . analysis consists in making experiments and observations and in drawing general *conclusions from them by induction*. . . .' (All italics mine.)

These passages suggest that Newton never suspected that his laws were anything but generalisations from experience. Furthermore, in the *Principia* he distinguishes between definitions and laws. Therefore the Laws (or Axioms) of Motion are obviously not intended as definitions and there should be procedures which make a test, in principle at least, possible. ('In principle': because we are here not concerned with the practical possibility of an experiment; its theoretical possibility is all that is required—we need only consider a 'thought-experiment.')

One of the experiments often brought up involves the use of Atwood's machine. Essentially it amounts to the application of a force to certain masses and the measurement of the resulting acceleration. However, the force applied is a weight. We are therefore presupposing the identification of weight and force on the one hand and the proportionality of mass and weight on the other. From a logical point of view and that of the *Principia* this remains, however, to be demonstrated. The demonstration involves the Second Law itself.

Now at this point adherents of the analytic view raise a powerful difficulty—generalising the result of this and many other similar experiments. To be meaningful, physical statements must contain terms which are measurable and defined independently of these statements themselves. And the trouble is that we do not appear to possess any means of measuring force and mass which do not at least seem to involve the Second Law itself (as well as certain other assumptions and laws, e.g. First and Third Law of Motion.) This would then constitute one reason for adopting the analytic view. However, perhaps it may be possible to avoid this conclusion by looking more carefully at Newton's own procedure in the *Principia*. For there he has prefixed his laws by certain definitions of mass and force. Definition I

[1] I. Newton, *Opticks*, Third edition, London, 1721, pp. 377, 380

tells us that mass is to be measured as the product of volume and density. Definition IV reads : ' An impressed force is an action exerted upon a body, in order to change its state, either of rest, or of moving uniformly forward in a right line.'[1] A short modern rendering of this would be roughly : ' Force produces acceleration.'

The analyst is not slow to notice certain objectionable features in these definitions. In contemporary physics ' density ' is measured as the quotient of mass and volume. It is true that in Newton's time density and specific gravity were synonymous terms and that density was one of the fundamental units, the density of water being arbitrarily taken as unity. Nevertheless, the definition remains clearly objectionable. It does, however, draw attention to the fact that the concept of mass is complex and an experiment which we shall describe below will suggest that there may have been reasons for this particular analysis of mass.

Definition IV looks similar to the Second Law and does not at first sight help to clarify the concept of force independently of it. Mach[2] goes so far as to say that it is ' wholly unnecessary tautology ' after having established acceleration as the measure of force, to say again that change of motion is proportional to the force. It would have been enough to say that the definitions premised were not arbitrary mathematical ones, but correspond to properties of bodies experimentally given. In that case the function of the Second Law would be no more than that of converting a nominal into a real definition. However, the Second Law and Definition IV are clearly not identical. A careful comparison will persuade us of this. The definition is couched in qualitative terms. The law tells us that the force is directly proportional to mass times acceleration. In Definition IV Newton clearly did not mean to make any reference to momentum. There is thus something left to be discovered. (True, Definitions V to VIII do look very similar to the Second Law, but actually refer to centripetal forces only. And they obviously do not function as definitions proper but rather as reminders of unfamiliar technical usages of certain terms.)

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[1] F. Cajori, op. cit., p. 2—Cajori actually translates ' . . . or of uniform motion in a right line.' Newton has ' vel movendi uniformiter in directum ' and thus does not intend to introduce ' motion ' (i.e. momentum, or mass × velocity) at this place. Cajori's translation slightly exaggerates the verbal similarity between the Second Law and Definition IV.

[2] E. Mach, *The Science of Mechanics*, 5th English edition, La Salle, 1942, Ch. 2, sect. 7, p. 302

The important thing about Definition IV, however, is that it summarises Newton's sweeping generalisation that forces determine accelerations. Acceleration is the fundamental aspect under which force is henceforth to be considered. The definition implies that it is possible to determine some circumstances in a complex situation in which a certain body (particle) is given a certain acceleration—this circumstance being called a force. Furthermore, this definition is fundamental; all others, for instance those of statics and engineering, become derivative within this system. And finally, the relative magnitude of the force is measured by this definition.

Now once you assume this much by definition (whatever may have suggested its usefulness) the proposition 'force is proportional to acceleration' (which makes up part of the Second Law) is true by definition, i.e. can be turned into a tautology through replacing the term 'force' by its definition.[1] The proposition is analytic. Of course, as already mentioned, the Second Law introduces another term as well, namely mass. Nevertheless, at first sight it appears difficult to see how we could test the prediction that for a given force the product of mass and acceleration is constant if we have no other measure of force but that of acceleration.

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All this certainly seems to provide a strong case for the adherents of the analytic view. And it seems to me that though Newton did not apparently realise it, supplementary considerations have to be introduced if the synthetic nature of the Second Law is even partially to be saved. To this end the believers in the synthetic view (the empiricists) sometimes attempt a defence through an enquiry into the *meaning* of force. Thus Broad[2] introduces a celebrated discussion of this problem by asking: 'What do we mean by force, and can we ever tell, apart from the laws of motion, whether forces are acting on a body or not?' If the Second Law is a 'substantial statement' (i.e. synthetic), and not merely a definition of force, we must 'already know what we mean by force, and already have a method of measuring its magnitude and direction.' Broad then continues:

> It seems clear to me that no one ever does *mean* or ever has *meant* by 'force' rate of change of momentum. It is certain that the Second

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[1] We have here assumed that Definition IV implies a direct proportionality of 'force' and acceleration.

[2] C. D. Broad, op. cit. pp. 161-62, 164

> Law, as originally stated, was not intended for a definition of force but for a substantial statement about it. Unquestionably the sensational basis of the scientific concept of force is the feelings of strain that we experience when we drag a heavy body along, or throw a stone, or bend a bow.

The objection to this is, of course, that the concept of force thus viewed is purely anthropomorphic. Nonetheless it is quite possible that the concept may be based on felt strains and the like, though our original sensations are vague and not very dependable and most of the forces dealt with in mechanics are not of this kind at all. It is difficult to base a scientific measure of force on felt strains. Broad points out that the same holds, however, of other scientific concepts, e.g. that of temperature. Here we define certain methods of measurement which, whilst agreeing roughly with a sensational basis, are more accurate and admit of a wider field of application. We employ for this purpose a thermometer. But—to quote Professor Broad once more—' no one . . . imagines that what we *mean* by temperature is the height of a column of mercury.' I am afraid, however, that some people do mean just this. Thus Bridgman,[1] the official spokesman for the ' operationalist ' theory of meaning or definition, writes : ' . . . just as the force concept was made more precise, so the temperature concept may be more or less divorced from its crude significance in terms of immediate sensation and be *given a more precise meaning*.' (Italics mine.)

What seems here largely involved is a decision as to the meaning of meaning, or more specifically, as to whether there are to be preferred meanings.

We may look at this matter perhaps somewhat more thoroughly. It will contribute towards greater clarity if we distinguish between a number of views that may be held about the meaning of force.

(A) *Radical Positivist View.*—This maintains that the only fact represented by the idea of force is that of an acceleration given to a certain body together with certain laws of nature determining the way different accelerations may be compounded or resolved.[2] On this view there is no sense in distinguishing between the proposition ' Force causes acceleration ' and the proposition ' Force is acceleration.' In more old-fashioned language we would say nothing ' real ' corresponds to the idea of force, or, the concept of force can be analysed without residue into certain observable spatio-temporal states of matter. In the solution of dynamical problems, for instance, the symbol for force is ultimately replaced by certain other variables, e.g. mass, length, and certain constants of nature. The hypothesis of a force which we use to understand such physical situations (unlike that of atoms and molecules) is not an hypothesis about the *existence* of anything but is an elliptical way of using certain propositions, e.g. the Second Law of Motion, to derive observable relations (Kepler's second law of planetary motion).

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[1] P.W. Bridgman, *The Logic of Modern Physics*, New York, 1947, Ch. III, p. 118

[2] See, for instance, J. Buchler (ed.), *The Philosophy of Peirce*, London, 1940, Ch. 3, pp. 33–36

(B) *Moderate Positivist View.*—This is similar to (A) but makes allowance for the empirical fact that forces may be measured by the behaviour of deformable bodies, for instance, as well as by accelerations. Here again, however, we are left with nothing other than correlations between the behaviour of an elastically deformable body and the dynamic behaviour of an accelerated particle.

(C) *Introspectionist View* (sometimes called ' metaphysical ').—The Introspectionist adds another bit of background to our knowledge of force (or to the use of the term force), viz, direct experience through muscular sensations. It might be thought at first sight that this does not add much to the concept of force : there is no distinction in principle between physical events happening in muscles and the nerve-centres connected with them and in deformable bodies like springs. The doctrine, however, means more than that : it holds that introspection supplies us with *direct* knowledge of force being exerted on or by us. It is this piece of information which gives meaning to the assertion ' a force *causes* a certain physical event to happen,' since (so it is held) we are here aware of being directly acquainted with something (= our will ; a decision to exert ourselves etc.) causing something else to change. This is not the place to enquire whether such evidence can be accepted as a basis for a causal argument—weighty objections have been raised against it since the time of Hume. The point is that the doctrine urges that this piece of knowledge or experience is at least sufficient to turn a sentence such as ' the application of a force is followed by an extension of a spring ' from an analytic into a synthetic one. It implies that we have sometimes extraneous knowledge of the action of a force ; in all other cases where we do not we may then assume hypothetically that something like a force is acting.

The issue between views (A) and (B) on the one hand and (C) on the other would in that case boil down to something like this : Can we say a spring is subject to a force in cases where we do not ourselves apply a force (e.g. pull at the end of a suspended spring) ? Certainly it is true that the spring does not experience a force, since it is an inanimate body. However, it might still be subject to one ? But suppose the pull of my hand be replaced by that of a body suspended at the end of a spring balance—the spring being stretched as before. May we not infer that the body exerts a force in the required sense of that word ? Now certainly I cannot have direct experience of a force exerted by an inanimate body whilst that body replaces my hand. I can only infer it, and this in two ways, either deductively or inductively. (*a*) Deductively—To do this we should have to assume (as a definition) that a spring is stretched only by a force. This leads us back to the positivist doctrine. (*b*) Inductive-hypothetically.—This is certainly possible though somewhat queer. It is queer because it is obvious that our inference is not to the existence of a special sort of entity at all. We should never expect to observe this sort of force in the way in which we expect eventually to isolate and observe under a microscope a germ whose existence we have assumed to account for a certain disease. At the back of the whole argument, I suspect, lies a certain theory of causation. We seek for a mediator, a third term, an explanation, connecting ' body suspended at the end of a spring-balance ' with ' stretched spring.' [1] We want to be able to say that the presence of the body is connected with something (a ' force,' an ' action,' an invisible cause of some kind or other) which *causes* the spring to stretch.

For these reasons it appears that it is largely a matter of choice—not affecting the facts as such—whether with the positivist you postulate that springs are only extended by what we *call* ' forces,' or whether you speak of an assumption of the *existence* of hypothetical forces. This fact explains why both views continue to retain their adherents.

Newton himself combined uneasily the various positions which later become separated out. In Definition IV he is certainly not identifying force and acceleration. Force is there ' an action exerted ' in order to change the velocity of a body. But at the end of Definition VIII he tells us that he is considering ' forces not physically but mathematically,' he is not ' defining their kind or manner of action.' Here he seems to be groping after a more positivistic interpretation. I think he never quite managed to make up his mind as to the logical status of the concept of force. In Definition IV the anthropomorphic element of action is still present making allowance for our stubborn feeling that acceleration is only one of the many manifestations of force. Elsewhere a more positivistic element is present : there we are merely interested in supplying a way in which forces are to be measured.

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[1] I am not considering ' gravitational fields,' etc since this would only complicate the issue whilst not changing the essentials.

Broad, as we have seen, gives preference to the anthropomorphic aspect of force. It is true that in this case a law like the Second Law becomes substantial, but it also becomes correspondingly imprecise. It seems that the requirement of quantitative precision produces progressively a demand for uniqueness of specification. It is the latter that involves us in difficulties which can clearly not be overcome by falling back upon the original imprecise and qualitative meaning of a concept. The two whilst genetically related are generically distinct. It is interesting to note that Broad, as a second alternative, does allow, though the Second Law is (as he holds) not a statement about what is meant by force, it may yet be one about ' how force is to be measured for scientific purposes.' Here our decision will depend upon whether the *only* way of measuring force is to measure the rate of change of momentum. Now Broad seems to think that there is at least one other way. If true, this would enable us to look at the Second Law as a sort of physical hypothesis or generalisation. This is precisely how the law has been used. For instance, Newton treating the Second Law as an empirical hypothesis infers from his pendulum experiment that, since different masses have identical constant accelerations towards the earth's centre, a constant force is acting the magnitude of which is proportional to the masses of the bodies concerned. Now, unfortunately, Broad's example, designed to give an alternative measurement of force, involves again the identification of weight and force. It is, however, important not to assume this so that Newton's proof just mentioned may proceed. And this proof involves, as shown, the Second Law. What was required was a notion of force independent of and prior to the Second Law.

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At this point it will be necessary to look more closely at the Second Law itself in order to see whether it does not contain sufficient synthetic features to make it at least plausible to have imagined that it is an empirically testable statement. This would explain our stubborn feeling that this law is more than a mere arbitrary definition. So far we have seen that any plausibility attaching to the synthetic view depends in part upon decisions as to preferential meanings and in part upon the type of dynamical system within which our concepts are considered.

To proceed, we remind ourselves that the First Law of Motion tells us what we are to understand by a 'free' particle, i.e. one not subject to any forces. Furthermore, we take Definition IV of the *Principia* to imply that fundamentally there is to be only one possible way of measuring forces. The method consists in assigning a numerical measure to any circumstance ('action') resulting in a 'free' particle becoming accelerated, the number so assigned being specified by the acceleration of the corresponding particle. Dealing with 'free particles' implies absence of gravitational, magnetic, frictional or any other forces. It is difficult to reproduce situations where such conditions are satisfied. However, as Mach has reminded us, 'when experimenting in thought, it is permissible to modify *unimportant* circumstances in order to bring out the features in a given case.'[1] As long as we are satisfied that the circumstances *are* unimportant the amount of idealisation implied is harmless. Moreover, the testing of *every* scientific law requires a certain amount of idealisation. Only, it must always be possible subsequently to discover whether the circumstances omitted *were* unimportant. It is sometimes thought that the *mere* introduction of abstraction and idealisation is sufficient to prove that a law is analytic. Such a view, however, would turn almost every scientific generalisation into a definition. The fact of idealisation can therefore not be used as a criterion to distinguish between those laws which are plainly empirical and those which are not.

And now to an experiment. As a standard model we picture a small rectangular body sliding upon a perfectly plane sheet of ice. The volume of the body v will have to be fairly small to approximate to an ideal mass-point. We now assume that this body may be 'pulled' by something, i.e. may be made to move with a certain acceleration which is to be constant. It is again assumed that the latter can be measured unambiguously. The body will then be subject to a certain force. That our body is subject to the application of a force has undoubtedly been suggested by the technical and sensational background of mechanics. Within Newton's system, however, we require nothing but Definition IV. Such conceptual purity would usually not be conducive to scientific advance. It is only important when we come to reflect upon the position of the concepts of a logical system once this has taken shape. Normally, it would of course be necessary to make use of many different kinds of definitions, some related directly to sensory experience, and—at the other end—some arising within a theoretical system as relations between a number of constructs.

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[1] E. Mach, op. cit. p. 341

We now require one more detail. Some method has to be devised through which to measure force otherwise than by acceleration. This will be necessary if we are to test the Second Law in a simple and straightforward experiment. According to this law, for a given acceleration, doubling the volume of a body entails doubling the force. As we saw, this proposition would be incapable of testing if force were merely measured as acceleration. Now it has usually been held that because Definition IV defines force in terms of acceleration we are for that reason prevented from proceeding at this point.[1] But it is obvious that no such thing is the case. Definition IV tells us when a force is acting. All we need is an alternative method to produce a measurable effect of such a force. We can then employ the numbers obtained by the use of the original definition to calibrate any other body affected by this force. An elastic thread or a spring will do. Although we already know that springs extend elastically under a pull, we need not presuppose this knowledge here ; i.e. we need not presuppose that pushes and pulls are the same sort of force as used in dynamics. We merely discover this. In other words, the static employment of force is here derived from the kinetic definition. Of course, again a certain amount of idealisation enters. We assume perfect elasticity ; furthermore, to each definite amount of force the spring will always assign one and only one definite number. Indeed, it may be said that all our method amounts to is to provide for a means of standardising the measurement of ' identical forces ' independently of the accelerations they produce.

We now imagine the following operations carried out. (*a*) Assume a certain homogeneous body of a certain material composition and a volume v_1. (Subscripts refer to the experiment.) We assume that the material composition is relevant to the result, i.e. that a certain mechanical property will correspond to it which we shall designate by r

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[1] If the Second Law is taken to be a definition of mass a similar difficulty would arise here.

(here r_1). We now attach a spring to this body and apply a constant force f_1 measured in the first place by a constant acceleration a_1. The spring is found to extend to a position which we mark e_1 (the position prior to application of the force being marked e_0).

(*b*) We now increase the magnitude of the force to result in an acceleration a_2, equal to say, $2a_1$. We mark the spring position e_2, and to it will correspond a force $f_2 = 2f_1$, by definition. The sole object of all this is the calibration of the spring so as to supply us with a force scale which is henceforth measured by the extension numbers e_i. Incidentally, however, we find that with the common type of elastic spring and for relatively low values of f, $e_2 = 2e_1$. But the force applied $f_2 = 2f_1$, as we saw. Hence we infer that the force applied is proportional to the extension of an elastic spring. This is equivalent to Hooke's Law which is here shown to be a consequence of our definition of force and an experiment. *Per contra*, if you assume Hooke's Law, the proposition ' force is proportional to acceleration ' becomes empirical. This is often done in certain demonstrations of the Second Law and is, of course, objectionable from a systematic point of view.

(*c*) We now return to an experimental consideration of mass. We shall not explicitly introduce the Third Law of Motion (' To every action there is always opposed an equal reaction ') though we shall have to see subsequently whether this law is not presupposed implicitly in our experiment.[1] Newton, as has been mentioned, defined mass as the product of volume and density. As yet, however, we have no means of measuring density though undoubtedly it refers to some property of the body other than its spatial extension (volume). We have denoted this property by r and hence may write the Second Law in the form $f = r.\, v.\, a$, assuming the usual units. It is in this form we proceed to test it.

We halve the volume of our test body, i.e. $v_3 = v_1/2$. We apply a force $f_3 = f_1$, say, all such forces now to be measured by our previously calibrated spring. Experimentally we find that $a_3 = 2a_1$. This part of the Second Law is thus empirical.

(*d*) We now take another body assumed to be of a different material constitution whose volume v_4 we make equal to v_1. We apply to it a force $f_4 = f_1$. We then find an acceleration $a_4 = a_1/2$, say. This suggests, on looking at the formula $f = r \,.\, v \,.\, a$, that r

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[1] Since Newton treated his three Laws as ' Axioms ' presumably he thought that they were independent of one another.

is some definitive property given by some experiment like the one just performed. Also, we infer that $r_4 = 2r_1$. We can then build up a coherent series of measurements in conformity with our formula. For instance, if we proceed to halve the volume of the body whose ' density ' $r = r_4$, we again find that with a force $f = f_1$ the acceleration is now doubled, and so on.

Our formula is, then, confirmed once again. But there is a difference between the third and fourth experiment and the conclusions derived. The third experiment showed that—on application of the same force—halving the volume causes doubling of the acceleration. In this instance, however, we possessed an independent means of measuring the volume of our test body. This is not the case in the fourth experiment. There are no ways, independent of the formula $f = r . v . a$, of measuring densities by elementary mechanical procedures. On the contrary, it is by means of an experiment like the fourth that one assigns numbers to densities (treating density as an inertial property). Undoubtedly the experiment enables us to *discover* that bodies of apparently different material constitution (as suggested by chemical or visual and tactual tests) possess different densities. But the direct proportionality of density to force is *assigned* by the Second Law and not proved inductively. The assignment of density numbers of course leads to a coherent series of values, i.e. for each density value the relationship holding between force, volume and acceleration of a body can be tested. The Second Law thus acts as a schema for the assignment of relative density numbers and thus indirectly of mass numbers.

5

The introduction of the properties of elastic bodies has of course been advocated before for the purpose of testing the Second Law, e.g. by such writers as Maxwell, Bridgman and Lenzen.[1] A celebrated effort in this direction was made by Andrade, in his ' Leçons de mechanique physique '—well known because of its discussion by Poincaré.[2] Poincaré raises several objections all with the purpose of proving that our law must be analytic. Not all forces are transmitted by threads and, moreover, to make them comparable they would have to be transmitted by identical threads. Nor are threads without mass and removed from all other forces. None of these appear to me to

[1] V. F. Lenzen, *The Nature of Physical Theory*, New York, 1931, Ch. 5

[2] H. Poincaré, op. cit. p. 104 ff.

be objections as such if you allow at all the possibility of an experimental test of any law. To take the last objection first. We meet here again the complaint that idealised conditions are never met with in nature. We have dealt with this before. As for the first objection the concepts which are needed to formulate a scientific law are always general. A concept like temperature is not only a generalisation covering a number of measurements with mercury thermometers but also with platinum resistance thermometers and so on. Lindsay and Margenau,[1] for a similar reason, object to the possibility of measuring forces by means of pushes and pulls. A push 'is always applied through a physical medium, and any attempt to measure it will have to be made in terms of the observed properties of such media. Since media differ widely, this is . . . not satisfactory. Surely we want a definition which is ultimately independent of the properties of special media.'

Now the question of definition of our major physical concepts is indeed a matter about which it is difficult to see clearly. It is certainly true that the history of physics bears witness to the attempt to arrive at ever more general definitions, i.e. those which define concepts by means of the most general laws. Thus temperature may be defined by the use of the expansive properties of metals or liquids. Subsequently we use the properties of gases that appear to be more 'ideally' suited for this purpose. A still later definition emerges as a consequence of the application of the Second Law of Thermodynamics. It is precisely for these reasons that some writers have felt that the operational method of scientific definitions is insufficient since it leaves the basis of the physical concept too narrow. As mentioned, an important concept usually requires more than one definition, and some of these will involve theoretical relations of a fairly general kind.[2] Yet all relations (i.e. laws) so employed are still ultimately empirical, however general they may be. They describe the behaviour of physical systems. The reference to 'special media' in your definitional statement can therefore never be an *ultimate* objection. It is a matter of practicality, not of principle.

Poincaré's objection involving the claim that we ought to use identical threads may be disposed of in a similar manner. Although we did actually imagine the use of one spring only it would in fact hardly matter if we had used several, provided we had first subjected them to the usual tests for similarity of performance. No more could be expected. Every scientific law implies a generalisation of performance, an extension over a field wider than that which provides its testing ground. All these strictures imply an ideal of metaphysical purity contradicting the very spirit of scientific procedure. Poincaré does make them presumably in order to indicate that the Second Law is analytic. But they can hardly count as reasons though they may have acted as causes for Poincaré's view. The only reason we have found so far for calling it analytic is that it does employ a restricted definition of force and serves to assign relative density numbers. At the same time we have also noticed that it is possible to put parts of the law to an empirical test. Before completing the discussion by extending these reflections, there is one last rather more powerful objection of Poincaré's which we must look at. To quote :

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[1] Lindsay and Margenau, op. cit. p. 91

[2] Cf. H. Margenau, *The Nature of Physical Reality*, New York, 1950, Ch 12, p. 232 ff. : 'Constitutive *vs.* Epistemic Definitions'

> But after all, what have we done ? We have defined the force to which the thread is subjected by the deformation undergone by this thread, which is reasonable enough ; we have further assumed that if a body is attached to this thread, the effort transmitted to it by the thread is equal to the action this body exercises on this thread ; after all, we have therefore used the principle of the equality of action and reaction, in considering it not as an experimental truth, but as the very definition of force.[1]

The strength of this objection, if justified, is this. We want to discover how much in the law is synthetic, how much analytic. By way of example ; we could have assumed Hooke's Law as a definition of force in which case the part of the law which at present rests on the assumption (definition) that force is proportional to acceleration would have become synthetic. We reject Hooke's Law as an assumption because we want to separate statics from kinetics. We also want to discover how far an independent verification of the Second Law is possible. The discovery that we presuppose the Third Law of Motion would not involve a mixture of statics and dynamics but it would show that a possibility of an independent verification was lessened. Whether this would also show that the law was analytic would depend upon whether the Third Law itself were synthetic and independent. The need for assumptions as such is, of course, irrelevant to the issue, except for showing that Newton's ' Axioms ' are not independent, and hence not axioms in the technical sense.

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[1] H. Poincaré, op. cit. p. 105

On the other hand, I am not certain whether Poincaré's objection need be accepted at all. Surely it is possible to consider one side of a force transaction at a time! What we are considering in the Second Law are not internal stresses but the effects of external forces. It is true, you may say, that it is assumed the force registered by the spring is the force transmitted, i.e. applied to the body. But in doing this I do not, so far, have to consider the reaction of the body upon the spring at all.

6

I conclude that our thought-experiment may be allowed to stand as a possible attempt to disentangle the rational and empirical features of the Second Law. Our experiment certainly does show that the law is not completely verifiable in the ordinary way. The force term in it can only be numerically specified given the defining characteristic of force, namely acceleration. This much Newton realised. Furthermore we have to introduce a derivative method (elastic deformations) to make sense of the experimental basis. This, whilst not explicitly mentioned by Newton, does not seem to involve a vicious circle as is sometimes supposed. Finally, the law acts as a schema in terms of which density may be given quantitative expression. On the other hand, it is an empirical fact that different bodies have different densities; and that for bodies of similar densities, equal forces produce accelerations inversely proportional to their volumes (cf. experiment (*c*) above).

It will be agreed, then, that the Second Law is hardly a straightforward definition. Nor is it simply true by definition. But at times the fact that it is not synthetic in a straightforward way has led some writers to call it a convention. (Its employment for the assignment of density numbers may strengthen such an idea.) It seems indeed peculiar to speak of convention in this connection at all. It is a convention to employ the symbol $\dot{y}$ for the first derivative of y with respect to the time variable. Or to take a slightly less clear-cut case, it is by convention that most statisticians work on a basis that a deviation of a mean of a sample from that of a normal population from which it is taken, which is greater than twice the standard error is significant. There are no doubt good reasons for agreeing to choose this limit of significance. But it would be perverse to speak of testing such a convention; and even perverse to ask whether the convention was in accordance with the facts—though it has its uses. The classical statement for the conventional doctrine is to be found in Poincaré, and it may be interesting to quote some of the things he says, because his views have been very influential.

> We see at the start a very particular and in sum rather crude experiment ; at the finish, a law perfectly general, perfectly precise, the certainty of which we regard as absolute. This certainty we ourselves have bestowed upon it voluntarily, so to speak, by looking upon it as a convention. Are the laws of acceleration . . .(etc.) . . . then only arbitrary conventions ? Convention, yes ; arbitrary, no ; they would be if we lost sight of the experiments which led the creators of the science to adopt them, and which, imperfect as they may be, suffice to justify them. . . . [1]

It is here implicitly admitted that there *are* experiments to back up the conventions. Furthermore, the only reasons for speaking of conventions at this place is that ' we regard (the laws) as absolute,' that we ' bestow certainty upon them.' It is not quite clear what this means. It may mean that the law is only approximately verified in nature. Or it may imply that we, on the basis of a few experiments, decide to *use* the law on other similar occasions also ; or that we shall employ it on occasions which differ significantly as an hypothesis. (Thus Newton infers the existence of central forces from Kepler's second law—*inter alia*—by means of assuming the First and Second Laws of Motion.) It may mean that we have decided to retain the law even in the face of contradictory experience, for instance where necessary by the introduction of hidden masses. All this, however, is only a matter of degree. We do not invest a law as such with additional strength merely by virtue of a decision. It looks as though all we have done is to convert the complaints mentioned before into a positive demand that the laws must be more than mere empirical generalisation of approximate value. However, the introduction of the term convention has the tendency to suggest a certain degree of petrification which arises out of the attempts to systematise scientific experience. Now the Second Law does actually function as one of the major axioms of classical dynamics. It is then understandable that we should try to reinforce its axiomatic character, its strength. Behind this lies a doubt connected with the difficulty of justifying inductive methods of proof and as its converse the ideal to exhibit the major principles of physics as necessary propositions. This is not the place to investigate such questions in general[1] but only the versions based on arguments discussed in this paper. (Nor should the latter be confused with quite a different doctrine of Eddington's which attempts to base the major principles of physics on—what he called—'epistemological *a priori*' propositions.)

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[1] H. Poincaré, op. cit. p. 106

In general, a closer inspection of the arguments that we have found adduced in favour of both the synthetic and the analytic views shows that the differences do not lie so much in the nature of the facts studied, but in differences of stress and emphasis placed on different aspects of the situation. The conclusions are arrived at so as to satisfy certain external claims, certain ideals of scientific method and knowledge. Thus we have found Broad stressing a particular pre-scientific meaning of force in order to turn the Second Law into a synthetic proposition. He wants it to be synthetic because this type of proposition is the only one reporting straightforwardly matters of fact and is thus closely linked with empirical reality. On the other hand, the ideal of precision and uniqueness of specification has the effect of turning the law into an analytic proposition. Similarly the attempt to avoid reference to special media in the definition of a scientific concept causes us to look for expressions of the greatest possible generality, and quite generally the demand for certainty leads us to regard important physical laws as conventions.[2]

So at one end we have the empirical basis or test. At the other a perfectly general law 'the certainty of which we regard as absolute.' The choice is ours. The empiricist will stress the concrete foundations of the law and continue to look at it as an hypothesis when using it beyond its foundations. The rationalist will stress the gap that divides the limited foundation from the universal result. He will therefore speak of the empirical basis as an exemplification rather than an inductive proof of a law, whose certainty must be regarded as absolute. He will turn the experimental truth into a definition. His justification for doing so is, however, still the usefulness of the definition. It is, therefore, not quite clear how much is in fact gained by the reformulation of logical terminology. The ambiguous nature of the empirical verification of the Second Law produces the conflict to an extreme degree and this explains the recurrence of the opposing views considered. It is in fact the same sort of conflict that we have found to lie at the bottom of the opposing doctrines about the meaning (nature) of force itself which was discussed above.

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[1] Cf. G. Buchdahl, 'Induction and Scientific Method,' *Mind*, Edinburgh, 1951, **60**, 16

[2] For certain purposes it would be necessary to distinguish more clearly between 'conventions' and analytic propositions. The latter are true because of the definitions of the concepts employed in their formulation. The former are true in virtue of public decisions. Similarly we should have to distinguish between analytic and *a priori* propositions. The latter are true not because of a chosen definition nor of a decision but because the evidence for their truth is not based upon external experience. It arises from some other 'necessity'—either of some intellectual intuition or of our thinking in general. A certain amount of confusion has been introduced by calling analytic propositions and conventions 'functional *a priori*' propositions.

In conclusion, we find that it is misleading to speak of the search for reasons which would prove the correctness of either the empirical or the rationalist viewpoint in borderline cases such as the one here considered. For certain purposes, for instance, some laws such as Boyle's Law are clearly synthetic, and a principle like the First Law of Motion, or the principle that ' Absolute motion cannot be observed ' have clearly an analytic use. Notwithstanding this, Boyle's Law may be regarded as part of the definition of an ideal gas ; and the principle of the observability of absolute motion has been thought to be testable inductively by the Michelson-Morley experiment. Conflict perhaps only arises where we attempt to reconcile the different ways in which certain scientific statements function, and where we try to use any particular way to prove the truth of some logical (or metaphysical) view conclusively.

G. Buchdahl

APPARENT MOTION AND THE MIND-BODY PROBLEM *

1

Psychological inquiry of the past few decades has produced what appears to be the most important evidence yet discovered regarding the spatial, temporal, and causal relations of perception, a psychological fact, to the body, a physical fact. The present study is of purely philosophical interest. It undertakes to relate this evidence, which concerns the phenomena of apparent motion, to the philosophy of mind-body. It proceeds from a description of apparent motion and its supposed underlying neurological correlates, and attempts to show that in terms of this current neurological theory and psychological data, a negative instance is presented to each of the following theories of mind-body relation: (i) psychoneural isomorphism, (ii) psychoneural parallelism, (iii) psychoneural double-aspect, (iv) psychoneural identity, and (v) psychoneural interaction. This conclusion must be regarded as tentative, reflecting the incomplete state of present knowledge of detailed cortical action.

2

It is commonly recognised that successive stationary stimuli, under proper spatio-temporal conditions, can give rise to the perception of motion. For example, two lamps of equal brightness, A and B, placed one centimeter apart and turned on and off successively within a temporal interval of approximately sixty milliseconds, will elicit from an observer the protocol statement that a *single* light has *moved* through the distance separating A and B. Under conditions such as these, it is impossible, on the sole basis of the protocol statements of observers, to distinguish the actual movement of a light from apparent movement. Such a distinction can be made only upon the experimenter's report of the stimulus conditions.

There are several different types of apparent movement. (*a*) We have just described a case of *optimal beta* movement: this occurs when a single light is seen to move laterally in one plane. (*b*) Under

* Received 13. ii. 51

appropriate conditions, a movement similar to optimal beta is observed, with the difference that the observer reports seeing 'movement' without seeing any identical object move: this type of apparent movement is called the *phi phenomenon.* (*c*) *Delta* movement would be observed in our original example if the second stimulus B were brighter than the first, A. Under such conditions the observer would report a single light stimulus moving *backward*, in one plane, from B to A. Here we have a phenomenal reversal of order from the stimulus conditions. (*d*) On the other hand, *bow* movement would occur in the same example under certain conditions if the observer were to report a single light swinging out of the plane of A and B toward him, describing an arc from A to B. (*e*) *Gamma* movement is the expansion-contraction effect observed when a single light source in a dark room is turned on and off, respectively.[1]

The phenomena just described provide a critical test for the traditional theories of neural action as related to visual perception. Our present conception of brain and nerve activity in vision is 'almost totally atomistic,'[2] assuming a point-to-point projection of retinal stimulation upon the visual cortex.[3] On this view, the areal character of perceived events is preserved on the neurolevel. For example, light waves reflected from the circular surface of a penny excite a circular retinal area. These excitations are conveyed to the striate area of the cortex, where presumably a circular area of neurons is activated. This circular area of neurons is conceived to be the neurological correlate of the perceived circle. A structural or geometric isomorphism is thus assumed to hold between objects as perceived, i.e. perceptions, and retinal and cortical configurations.

The phenomena of apparent motion, however, cannot be assimilated into an areal point-to-point schema, for at least two reasons: (i) there is believed to be no retinal stimulation, for example, issuing from between points A and B which can be projected upon the visual cortex and correlated with the moving object as perceived,[4] and (ii) discrete nerve impulses, stimulating individual cortical cells, do not provide a continuous field on the neurolevel to correspond to the continuity perceived on the psycholevel.

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[1] This is not a complete list. For a comprehensive summary of these phenomena, see Walter S. Neff, 'A Critical Investigation of the Visual Apprehension of Movement,' *Amer. J. Psychol.*, 1936, **48**, 1-42.

[2] S. H. Bartley, *Vision*, New York, 1941, p. 336

[3] G. H. Bishop, 'Electrical Responses Accompanying Activity of the Optic Pathway,' *Arch. Ophthal.*, Chicago, 1935, **14**, 992-1019

[4] References to 'A' and 'B' refer to the example above, and will be employed in this manner throughout the text.

The Gestalt psychologists, as is well known, originally focused attention upon this problem, and have attempted to provide neurological theory to explain it. In general, Gestalt theory answers the two difficulties given above as follows : (i) although in apparent motion there is no retinal stimulation corresponding to points between A and B, and hence no related cortical excitation (in terms of atomistic, point-to-point neurological theory) directly correlated with the perception of apparent movement, the brain operates as a *field*, a continuous medium, and not as a bundle of neurons, and is capable of ' fusing ' discrete excitations : in this way it is said to supply a neurolevel in one-one formal or structural relation to the psycholevel ; (ii) although the nerve impulses arising upon presentation of single stimuli may be discriminable, they are not isolated events which are in chain-like relation to isolated cortical events. Rather the optic sector is said to react as a whole, and so does the brain. There is allegedly no neural *discontinuity* on the Gestalt view, and thus the problem of deriving a perception of motion from discrete neural events is held to be solved.

The first of the Gestalt theories on this problem appeared in 1912 when Wertheimer postulated what he called ' cerebral cross-processes ' (*querfunktionen*) on the neurolevel as the brain correlate for perceived motion.[1] He suggested that when any stimulus such as A is transmitted to the visual cortex, a ' circle of excitation ' spreads from the neural centre *a*, directly stimulated. If now stimuli A and B are given successively, two corresponding neural areas, *a* and *b*, radiate circles of excitation. If *a* is stimulated prior to *b*, a ' short-circuit ' occurs, and a spread of excitations *crosses* from the neural area excited by A to that excited by B. This cross-process is the *cause*, or neural correlate, of the perception of apparent motion. In this theory Wertheimer thought he was able to provide continuous neural activity to correspond to the perceived continuity of apparent motion, and thus to show the structural similarity of perceptual and neurological events.

However, Wertheimer's explanation faces at least four serious difficulties : (A) It fails to specify that the successive points stimulated in the brain, and the resulting ' cross-process,' must all occur *before* the perception of motion commences. This provision is necessary,

[1] Max Wertheimer, ' Experimentelle Studien ueber das Sehen von Bewegung,' *Zeits. f. Psychol.*, 1921, **61**, 161-265

since what is perceived in apparent motion is not (i) light A and a circle of radiation, then (ii) light B and another circle of radiation, and finally (iii) the ' cross-process ' or ' short-circuit ' of the radiations from A to B. In optimal apparent motion what is perceived is a single moving object, while on Wertheimer's explanation we actually have three discrete neural events. This view therefore requires that the total neurological sequence must occur before the perception of motion begins. This is necessary, for the *direction* of motion as perceived is determined by the second and last stimulus, which must therefore reach the visual cortex prior to the beginning of the psychoevent correlated with it. If provision is made for this requirement, we will have two time orders, a physical time for neuroevents, which precedes a psychological time for psychoevents.

(B) The second difficulty facing Wertheimer's theory is that the *order* in which neuroevents occur is different from the perceived order of apparent motion. This is seen in the following considerations: we have ordered above the neuroevents corresponding to apparent movement from one to three as follows, (i) cerebral excitation *a*, and *a*'s radiation, (ii) excitation *b* and *b*'s radiation, and (iii) the cross-process from *a* to *b*. Correspondingly we may number the perceived motion as (1)—(2)—(3), to indicate (1) the beginning of movement at A, (2) the intervening motion from A to B, and (3) the ending at B. Now if we correlate the neuroevents with the psychoevents, we get :

Psychoevents : (1)—(2)—(3)
Neuroevents : (i)—(iii)—(ii)

It will be noticed from this schema that the third and last event on the neurolevel, the cross-process, occurs secondly for perception, while the third and last event of the psycholevel occurs secondly on the neurolevel.[1] This poses a causal conundrum for the theory : normally when we have a series of causes related to a series of effects, we find the following similarity of order in the two sequences, X, Y, Z,—*x*, *y*, *z*. Now in the diagram above, since (i) is the cause of (1), (ii) the cause of (3), and (iii) the cause of (2), it would seem that we would not be able to hold, according to the generally accepted view that causes cannot follow their effects, that such an inversion can occur. However, we might say that the total series of neuroevents is completed before any psychoevents occur. But it would require that we consider this as a special case of causal relation.

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[1] This criticism is directed primarily at the theory of Wertheimer, which analyses the cerebral correlates of the perception of optimal apparent motion in terms of two points of excitation in the cortex, followed by the cross-process which connects them. It is easily possible, on the other hand, to specify the conditions for a cerebral mechanism which will provide for the occurrence of the cross-process *before* the excitation of point *b* occurs. Under such a condition, we would have excitation *a*, cross-process, and excitation *b*. This emendation of the theory, however, will not resolve the problem of the diversity between neuroevents and psychoevents in apparent motion. For we would still have the problem of continuity between excitation points *a* and the cross-process, and *b* and the cross-process ; which is the problem of the rôle that isolated or discrete neuroevents play in the so-called ' field theory,' as well as the problem of atomism.

(C) Since Wertheimer's theory analyses the neural correlates of the perception of optimal beta movement into three discrete events (two centres of excitation and the cross-process) it is subject to the same objections as the atomistic view in regard to the matter of continuity. For it is clear that we have no continuity, for example, between *a* and the cross-process and between the cross-process and *b*. *Thus the whole problem appears anew on a different level of analysis.*

(D) Finally, some neuroevents occur that have no corresponding psychoevents. We do not perceive, as was stated above, two discrete stimuli, A and B, begin to radiate and then ' short-circuit ' or ' fuse '. We perceive rather a total process that has a different temporal and spatial structure than is supposed to occur on the neurolevel.

One correction of importance to the theory of Wertheimer has been suggested by Köhler. He assumes that ' the optic sector reacts as a whole to the whole constellation of physical stimuli',[1] and further, that the fusion of the cortical points *a* and *b* first occurs neurologically, while registering later on the psycholevel so that ' the conscious process arises as a unitary structure.'[2] This emendation takes care of the difficulty discussed above under heading (A), i.e. that points *a* and *b* in the cortex must be stimulated before the perception of motion begins.

Köhler further makes explicit the relation he conceives to exist between the brain and psychological events. He claims that the psychological relation is ' isomorphic ' : ' mental operations and their neural counterparts must structurally resemble each other.'[3] This view of psychophysical isomorphism may be expressed more precisely as follows : there is a one-one relationship of structural similarity, with qualitative and spatio-temporal diversity, between neuroevents and psychoevents, the latter being both later in time and causally dependent upon the former. In this statement of isomorphism we may consider the term 'structural similarity' to be replaceable by 'similarity of temporal and spatial order,' while the expression 'spatio-temporal diversity' is intended to mean 'not in the same space-time locus.'[1]

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[1] H. Helson, ' The psychology of Gestalt,' *Amer. J. Psychol.*, 1925, **36**, 517

[2] Loc. cit.

[3] Wolfgang Köhler, *The Place of Value in a World of Facts*, New York, 1938, p. 396

The following quotation from Köhler may help to make this clear: If an image of . . . [an elephant] is projected upon my retina, cortical processes within a circumscribed region of my brain are immediately segregated as a particular microscopic unit, which is my 'psychophysical elephant'; and one phenomenal thing, the elephant percept, appears in my visual field.[2]

In regard to apparent motion Köhler speaks of 'fusion' instead of 'cross-processes.' For our purposes this is but a terminological difference from Wertheimer.[3] The notion referred to, again, is that the two excitation points in the cortex, *a* and *b*, come together or spread in some fashion to cover the intervening area. This seems to be required, if the continuity of perceived motion is to be formally correlated with and derived from neural action.

The foregoing is a general account of the direction that current physiological theories have taken in an attempt to provide an explanatory framework for the phenomena of apparent motion. The alternative views at present are the field and atomistic physiological theories, while the experimental evidence is predominantly atomistic,. as indicated in the following summary from Bishop:

> While the mechanism of vision can in no respect be finally explained at present, a provisional summary of the activities of its parts, so far as these are known at present, may be pieced together. . . . Even a short flash of light on the retina of sufficient intensity, after a latent period during which the sensory cells respond, causes a repetitive response in the fibres of the optic nerve. Just before and during this response, neural elements in the retina give rise during their activity to the typical retinal potentials. At this level interaction takes place between different pathways, to the extent that activation in one pathway facilitates and speeds up the activity of another similarly stimulated,

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[1] All space-time references in this essay are absolute (Newtonian), not relative (Einsteinian). That is to say, one homogeneous time and one homogeneous space are assumed, without regard to operational considerations for their specifications,

[2] Loc. cit. p. 219

[3] There are significant differences in Köhler's hypothesis of the brain as an electromagnetic field from Wertheimer's view. But while this is an important part of Köhler's theory, it does not affect our present problem.

> although there is no evidence that one pathway is actually caused to respond by the activity of an adjacent one unless it is stimulated as well.
>
> The successive responses of the optic nerve fibres on reaching the thalamus are there summated both temporally and spatially (with the previous proviso) in such a way that the greater number of fibres activated or the more successive responses in each fibre, the shorter the latency of cortical response. It may be inferred that aside from this type of facilitation the spatial pattern of fibre response corresponds to visual pattern (form, etc.), while the frequency and number of responses from a given area of the retina corresponds to the intensity of sensory effect or brightness. . . . Nothing in this course of events can be assigned at present as the basis for colour discrimination. . . .[1]

The physiological atomism indicated in this summary does not provide for structural similarity between neuroevents and psychoevents in apparent motion, as would seem to be necessary if we are to have direct predictability from the neurolevel to the psycholevel. For an atomistic, point-to-point view of the projection of retinal stimuli upon the visual cortex allows no direct prediction of perceived motion that arises from spatially separated, discrete stimuli. Such a prediction requires structural similarity, and hence continuity, on the neurolevel, and this means a field theory. However, the field theory of the Gestalt psychologists, while providing continuity on the neurolevel, does not provide for the assimilation of isolated neural events into this continuity, and does not provide for structural isomorphism between neuroevents and psychoevents in apparent motion, as its proponents have claimed. This criticism, which will be amplified in what follows, in effect means that neither the atomistic nor the field theory is adequate, as presently formulated, to solve the problem of direct predictability from the neurolevel to the psycholevel in apparent motion.

3

In discussing Wertheimer's theory of cross-process, we stated that there are four objections to this view, if it is regarded as providing a ground of neural activity concurrent in time and structurally similar in space to perceived events : (A) Since the posited neuroevents are not structurally similar to the psychoevent which is the perception of apparent motion, if in fact they do occur, they must precede the perception ; (B) The order of neuroevents will thus be different from the order of psychoevents; (C) Since the theory includes isolated events it is subject to the same criticisms as the atomistic view; and (D) Some neuroevents will occur which have no perceptual counterparts (i.e. the 'fusion' of discrete cortical events has no direct perceptual counterpart).[1]

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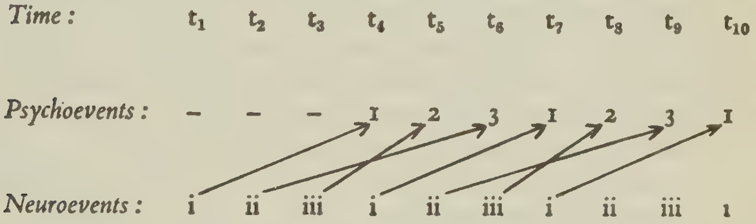
[1] G. H. Bishop, 'Electrical Responses Accompanying Activity of the Optic Pathway,' *Arch. Ophthal.*, 1935, 14, 1016–17

These difficulties may be restated more generally as follows: (*a*) There is a temporal diversity between neuroevents and psychoevents, the latter succeeding and being causally dependent upon the former; (*b*) There is a spatial diversity between neuroevents and psychoevents; and (*c*) There is a structural diversity between neuroevents and psychoevents.

One further point must be noted concerning the causal relation between neuroevents and psychoevents in apparent motion. Using the field theory as a basis of explanation, in successive perceptions of apparent motion we have the following:



Time:
t_1 t_2 t_3 t_4 t_5 t_6 t_7 t_8 t_9 t_{10}
Psychoevents:
– – – 1 2 3 1 2 3 1
Neuroevents:
i ii iii i ii iii i ii iii i

Since, for reasons developed above, it seems that all of the neuroevents must be completed prior to the psychoevents in a perception of apparent motion, we have a continuous series of neuroevents preceding in time a continuous series of psychoevents. Now if it is granted that the psychoevents are causally dependent upon the neuroevents, and no one competent to judge would doubt that this is so, then we may say that (*d*) the psychoevents which are perceptions of apparent motion cannot causally act upon the neuroevents correlated with them. This point is illustrated in the diagram above. At time t_4, for example, on the psycholevel, we have a correlated neuroevent occurring that will be the cause of a psychoevent at time t_7. As regards successive perceptions of apparent motion, this would rule out the possibility of interaction between the neurolevel and the psycholevel, for one of the necessary conditions of causality is precedence in time.

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[1] The principal difficulties in the field theory will apply *a fortiori* to the atomistic view, so we need not consider it separately.

4

The foregoing considerations may now be recognised as negative instances to the following theories of mind-body relation :

(i) *Psychoneural isomorphism :* There is a one-one relationship of structural similarity, with qualitative and spatio-temporal diversity, between neuroevents and psychoevents, the latter being both later in time and causally dependent upon the former.

This theory is negated in virtue of (*c*) above : there is a structural diversity between neuroevents and psychoevents.

(ii) *Psychoneural double-aspect :* There is a spatio-temporal identity with qualitative diversity between neuroevents and psychoevents which are not in causal relation.

This theory is negated in virtue of (*a*) and (*b*) above : There is a temporal diversity between neuroevents and psychoevents, the latter being causally dependent upon the former. There is a spatial diversity between neuroevents and psychoevents.

(iii) *Psychoneural parallelism :* There is a structural, qualitative, and spatial diversity between neuroevents and psychoevents which are in one-one temporal relation but not in causal relation.

This theory is negated in virtue of (*d*) above : there is a temporal diversity between neuroevents and psychoevents, the latter being causally dependent upon the former.

(iv) *Psychoneural identity :* There is a spatio-temporal and qualitative identity of neuroevents and psychoevents which are not in causal relation.

This theory is negated in virtue of (*a*) and (*b*) above as in (iii).

(v) *Psychoneural interaction* : There is a structural, qualitative, and spatio-temporal diversity between neuroevents and psychoevents, the latter both causally acting and being acted upon by the former.

This theory is negated in virtue of (*a*) above : The psychoevents which are perceptions of apparent motion cannot causally act upon the neuroevents correlated with them.

Objections may be raised against this claim that we have herewith presented a negative instance to the interaction theory, since in our statement of the theory we have not differentiated between perception and volition. For the interaction theory is customarily stated in some form which has the neurolevel acting upon the psycholevel in perception, and the psycholevel acting upon the neurolevel in volition. However, in order to make good the latter claim, it would be necessary to show that in volition there are no neurological causal antecedents. However, present evidence seems to indicate the opposite, e.g. certain types of goal-directed behaviour are generally acknowledged to result from definite bodily needs such as hunger and thirst.

Further, if in fact events on the neurolevel in the perception of motion do precede in time events on the psycholevel, and the present evidence for this view is strong, then on general physiological grounds there is good probability that the same is true for other types of perceptual events. That is to say, we are suggesting that the present evidence from apparent motion may be generalised in the form of an hypothesis about the temporal relation between any neuroevent and any psychoevent, i.e. all psychoevents follow in time their related neuroevents.

At present it is difficult to obtain sufficient detailed direct evidence on this point, but we may cite one further case of indirect evidence from experimental psychology, which gives added weight to the present interpretation that the temporal characteristics of the relation between neuroevents and psychoevents in apparent motion generally characterise the relation between neuroevents and psychoevents. This concerns the well-known 'startle' reaction, in which a stimulus in the form of a loud or unusual noise causes an observer visibly to 'jump' or 'startle.'

The customary report of this experience places the perception of the bodily reaction of startle *prior* in time to the perception of the auditory stimulus itself. Now if the arrival of this stimulus on the neurolevel were identical in time with the perception of the sound on the psycholevel, then it would not be possible to perceive the bodily reaction of startle *prior* to the perception of the stimulus which excited it. For it is known that the auditory stimulus goes to the higher brain centres, where it excites the bodily reaction. In this case it seems quite clear that the perception of sound on the psycholevel follows in time the pattern of cortical excitation on the neurolevel.

5

We have undertaken to relate in this paper factual evidence from experimental psychology and physiology to neurological and philosophical theory. The factual evidence from psychology, based upon the protocols of observers and experimenters in experimental contexts, may be summarised generally as follows: Successive stationary stimuli, under proper spatio-temporal conditions, give rise to the perception of motion.

The factual evidence from physiology concerns the nature of retinal stimulation, nerve conduction, and brain excitation in vision. In general, this evidence seems to justify a modified point-to-point view of retinal projection upon the visual cortex.[1] The theoretical problem suggested by this evidence, when applied to the phenomena of apparent motion, has led to the development of a field theory of neural action, which is designed to allow direct predictability from the neurolevel to the psycholevel in apparent motion.

It has been the contention of the preceding analysis that both the atomistic and field theories of neural action provide grounds for asserting the following four propositions concerning the relation between neuroevents and psychoevents in the phenomena of apparent motion :

(*a*) There is a temporal diversity between neuroevents and psychoevents, the latter succeeding in time and being causally dependent upon the former.

(*b*) There is a spatial diversity between neuroevents and psychoevents.

(*c*) There is a structural diversity between neuroevents and psychoevents.

(*d*) Psychoevents which are perceptions of apparent motion cannot causally act upon the neuroevents correlated with them.

It has further been maintained that these four propositions provide negative instances to the following five theories of mind-body relation : psychoneural isomorphism, double-aspect, parallelism, identity, and interaction.

We have thus eliminated all but one of the classical philosophic theories of mind-body relation, viz, psychoneural epiphenomenalism.[2]

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[1] With the exception of facilitation, cf. above, pp. 241-2

[2] We have not included in the theories herein discussed the ' Nerve Net Theory of Consciousness ' originated by Culbertson. At first sight it appears that this theory might be classifiable as epiphenomenalistic, but there are some unique features of Culbertson's view that make it at present seem advisable to analyse it apart from the classical theories that we have been considering. See James T. Culbertson, ' Distances and Qualities in Perceptual Space,' *J. of Psychol.*, 1945, **19**, 109-132 ; and ' Temporal Change in Perceptual Space,' ibid. 1946, **21**, 3-23 ; for a detailed account of the entire theory, see his book *Consciousness and Behavior*, Dubuque, 1951.

This theory may be stated as follows: There is a structural, qualitative, and spatio-temporal diversity between neuroevents and psychoevents, the latter succeeding in time and being causally dependent upon the former. If this theory is examined in regard to the four propositions cited above, it alone will be seen to be compatible with them. It appears, therefore, on the present incomplete and tentative evidence from the experimental sciences (a point we wish to emphasise) that of the mind-body theories formulated to date, the only currently tenable view is *psychoneural epiphenomenalism.*

George Watson

DISCUSSIONS

The Homeostat

In his paper 'The Hypothesis of Cybernetics' (this *Journal*, 1951, 2, 12) Dr Wisdom claims that Professor W. R. Ashby's *homeostat* '. . . holds out the very real possibility of making a machine capable of performing operations . . . beyond the knowledge of the designer.'

This is a very important claim indeed for, as Dr Wisdom notes, it is a refutation of Descartes' principle that there must be at least as much reality and perfection in the cause as in the effect ; it may therefore be advisable to examine more closely its factual foundations.

From the original description in *Electronic Engineering*, and from various subsequent descriptions including Dr Wisdom's, it is hard to understand just where the *homeostat* differs from an analogue type of calculating machine capable of producing a large number of sets of solutions to an equation with a large number of variables. It does this by the process of altering, more or less arbitrarily, one or more of the variables, and then discovering at least one set of values for the remaining variables such that its 'fundamental equation of stability' is satisfied.

If some external agency instead alters a variable (or even removes it from the equation), then different sets of solutions to the new equation are obtained and ultimately, if all but one variable are removed, the remaining one can no longer have any but a single fixed value in the stability equation.

Now although a complete understanding of this sort of process may in some cases be 'beyond the knowledge of the designer,' it will be so only because the number of sets of solutions to the stability equation is infinite, that is, the interrelation between two or more of the variables allows an infinite number of variations in one to be compensated by an infinite number of 'opposite' variations in another. It will not, however, be beyond the knowledge of the designer to appreciate this possibility, or rationally to explain why it occurs.

It may be that Professor Ashby's machine contains some such infinite variety of possible solutions, carefully included in its design by the forethought of its designer. Why this should make the *homeostat* more like a human brain than any other calculating machine is something which perhaps Dr Wisdom could explain more fully.

B. M. Adkins

The Hypothesis of Cybernetics

Dr Wisdom in his excellent article on 'The Hypothesis of Cybernetics' (this *Journal*, May, 1951), has concerned himself with those aspects of the theory which have a biological significance and has not discussed in detail the claim that digital computors can simulate human thought activity. I wonder if I may therefore make a few remarks on this matter.

I do not wish to deny that digital computors can be made to perform logical and mathematical computations, which are certainly a form of thinking when performed by human beings. Man has from the dawn of civilisation introduced devices to enable him to perform complicated calculations in a semi-automatic way, e.g. the abacus. The written arabic notation is a visual model of this instrument, the physical operations on the abacus being replaced by written operations on paper. In principle, modern digital computors might fairly be described as electrified abacuses working to the base 2, electrical pulses being substituted for the beads of the counting frame.

The introduction of a visual symbolism lessens the amount of thinking we do; we can depict pictorially not only our numerical concepts, but also the operations performed upon them. Computations can now be carried out visually on paper instead of in our heads, which is roughly what we mean by a calculus. In a sense, a calculus is the very antithesis of thinking, since it is a mechanical routine substituted for our intuitive and often vague and imprecise thought processes. By applying the rules of the system very complicated and long chains of deductions can be made with a minimum of thought and effort. Our concepts are translated into the basic signs of the calculus, operations are performed upon these signs and they are then retranslated back again. In effect, we have produced a simple logical machine, which when premises are inserted, grinds out conclusions according to the laws of logic. There is little that is new in this approach; what is new is the speed and large measure of control with which these machines perform their computations.

But, if we grant that these machines are complex pieces of symbolism, a development of the visual aids we have known for centuries, it is clear that in order to acquire a significance the symbols need to be linked with a set of referents, in this case logical and mathematical concepts. Neglect of the pragmatic or instrumental aspect of such machines leads us to attribute to them a capacity for thinking which they only have by proxy. As symbolic devices they merely represent and transform our logical and mathematical concepts, facilitating their mechanical handling. The transformation of formulae according to a fixed set of logical rules is not, however, a sufficient criterion of thinking. Unless the resultant formulae or patterns of symbols are translated in terms of their referents, the transformations remain a meaningless array of physical marks. It does not therefore make sense to talk about such machines ' thinking ' without an intelligence which can read off the collocation of symbols produced by the machine. When analogies are drawn between computing machines and minds, this instrumental aspect tends to be overlooked ; one can conveniently forget this aspect in purely formal discussions of logical calculi, but once these machines are used for drawing psychological conclusions about man himself, the whole argument reduces itself to a tautology. Such machines cannot be said to be thinking unless there is some one to interpret the end-result (whether given in code or typed in ordinary English), which is certainly a form of thinking.

The suggestion that the mind or brain is itself a computing device which models by means of its symbolism the external world, seems to be an attempt to describe the mind in terms of a very limited and special type of its own symbolism, that of the logic and mathematics. What were at first devices to aid numerical thinking, have now become identified with the whole thought and mind itself. Logical and mathematical symbolism is, however, *sui generis ;* by far the largest part of the symbolism used in everyday discourse is not thus sharply defined and has numerous emotive overtones. This is especially the case in poetic symbolism and in the workings of the unconscious mind. The thesis that logic and thought are one, cannot be sustained ; indeed one might say that it is almost a natural feature of the mind for its symbols to be vague and ambiguous, rather than clear-cut and unequivocal. Such machines are then really concerned with a highly artificial form of language, with a set of rules for transforming one set of expressions into the other. The manipulation of strings of symbols according to the rules of a specific calculus is now taken to be identical with thinking itself. There seems to be a confusion between logic and psychology here. Logic has only to do with the truth and falsity of our arguments, and has little if anything to do with the way we actually think about them.

W. MAYS

REVIEWS

THE COMPARATIVE METHOD IN THE STUDY OF BEHAVIOUR[1]

THOSE who attended the 1949 Symposium of the Society for Experimental Biology on Physiological Mechanisms in Animal Behaviour were impressed by the wide variety of disciplines represented by the speakers and by the members of the audience. The fact that the participants were able to communicate so effectively with each other indicated not only that the various disciplines overlap in interests but also that they have certain common methods for attacking their problems. These common methods are based upon strategies or logical procedures of research design, to which all sciences subscribe, even though the tactics or specific techniques employed in putting the strategies into effect may differ very significantly from one discipline to another. Of particular interest to this Symposium was that strategy usually referred to as 'the comparative method.'

The recent publication of the reports presented at this Symposium shows clearly how the comparative method, supplemented by other scientific methods, may serve to unify the study of animal behaviour. A paper by K. Z. Lorenz, devoted to a discussion of the use of this method in studying innate behaviour patterns, is of particular interest since it speaks out boldly against the misuse of the comparative approach and, in so doing, discusses the nature of this procedure as it is employed in Comparative Ethology. According to Lorenz :

> Since the days of Charles Darwin the term 'comparative' has assumed a very definite meaning. It indicates a certain rather complicated method of procedure which, by studying the similarities and dissimilarities of homologous characters of allied forms, simultaneously obtains indications as to the phyletic relationships of these forms of life and as to the historical origin of the homologous characters in question.

Since the major contributions of this Symposium to our knowledge of animal behaviour are to a great extent dependent upon the use of the comparative method, and since other reviews have already discussed the nature of these contributions, it is the intent of the present paper to examine the comparative method from the point of view of its application in a science of behaviour.

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[1] A consideration of the requirements of the method in its application to the study of behaviour, particularly as exemplified in research reported in the *Symposia of the Society for Experimental Biology, No. IV, Physiological Mechanisms in Animal Behaviour*, Cambridge University Press, 1950, pp. vii + 482. 35s.

Long before the days of Charles Darwin[1] the procedures of the early 'comparative' sciences had developed certain common characteristics, collectively referred to as 'the comparative method.' In comparative anatomy, one of the oldest of the comparative sciences, these common characteristics have been described in the following terms[2]: 'the comparative method is based on a critical comparison of structures that are truly homologous rather than analogous. Homology deals with those structures that are alike in origin and fundamental structure. Analogy deals with structures that are alike in function and sometimes superficially alike in form but different in origin.' This statement of the method sets certain key requirements which have been clearly met in the older comparative sciences but which present much more difficult problems to the student of behaviour, in either its individual or social aspects. Those who would employ the comparative method in studying behaviour of living organisms must concern themselves with the kind of comparisons to be made, with procedures for describing the 'structure' of behaviour, and with means for distinguishing between 'homologous' and 'analogous' behaviour patterns.

'Comparison' is a characteristic of all scientific methods and cannot alone serve to distinguish among them. Comparison may lead to classification, to the discovery of an evolutionary sequence, or to an application of other inductive methods of science in establishing causal relations. From this point of view[3]: '. . . the term Comparative Method is little more than a vague name for *any* scientific method.' Whenever it is used in the comparative biological sciences 'comparison' implies the study of at least two forms of organism and in practice it has usually meant the study of a wide diversity of forms. Students of behaviour, particularly psychologists, have frequently disregarded this requirement. As one member of the Symposium has stated elsewhere[4]: 'There are remarkably few comparative studies . . . of behaviour. A great part are meaningless . . . because the tests were made with a single species and comparable data are available for no other.' This has led to an animal psychology rather than a comparative psychology.

References to 'species' in this criticism and to 'allied forms' in Lorenz's definition imply that comparative studies of behaviour are based on groups

[1] C. Singer, *A History of Biology*, London, 1950

[2] L. A. Adams and S. Eddy, *Comparative Anatomy: An Introduction to the Vertebrates*, New York, 1949

[3] A. Wolf, *Textbook of Logic*, London, 1938

[4] K. S. Lashley, *Quart. Rev. Biol.*, 1949, 24, 28-42

differing phyletically. This has been true of many comparative studies but is not a prerequisite for application of the comparative method. Comparisons may be made between individuals of the same species differing in other characteristics. Thus certain researchers have observed similarities and dissimilarities in the behaviour of individuals differing in ' constitutional type.' Physiologists have contrasted changes in the bodily processes of ' normal,' ' neurotic ' and ' psychotic ' human subjects under identical conditions of stress. Social anthropologists have applied the comparative method to the study of behaviour in different cultural groups and sociologists to groups differing in socio-economic status.

Use of the comparative method requires that the behaviour patterns studied must be alike in both origin and structure. Structure is described in terms of the stimulus-organism-response relations involved in the behaviour pattern. Lorenz has referred to such description as leading to ' a morphology of behaviour ' and constituting a ' methodologically first task ' in the comparative study of behaviour. In some instances, e.g. reflex responses, this relationship may be one in which the stimulus plays the most predominant role. In others, e.g. complex habits, the level of maturation, the past experiences, and the present motivational state of the organism may be the determining factors in whether or not the response will appear and the form it takes if it does appear. In still other instances, as G. P. Wells [1] has shown, the behaviour pattern, although modified by varying stimulus conditions, is basically dependent on the action of internal pacemaker centres. The description of the ' structure ' of a behaviour pattern in terms of stimulus-organism-response relations is illustrated in many of the Symposium's reports. In P. H. T. Hartley's paper on the ' Experimental Analysis of Interspecific Recognition ' the frequency and nature of ' mobbing behaviour ' in various species of wild birds are described as functions of the characteristics of the stimulus-object which elicited the responses. Konorski contrasts conditioned responses of the ' first- ' and ' second-type ' primarily in terms of stimulus-response-reinforcement relations. Tinbergen suggests that evidence for a hierarchial organisation of nervous mechanisms underlying instinctive behaviour comes from observations of the interrelations between certain activities, all of which ' . . . are partly dependent on internal impulses and partly upon external stimuli.' The importance of description in terms of stimulus-organism-response relations has been stressed by Warden and Warner [2] in their discussion of the development of procedures for studying behaviour : ' . . . the proper goal of the objective comparative psychology is the determination of the conditions and laws of stimulation-response processes, involved in the continuous interaction between the organism and its environment.'

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[1] *Symposia*, op. cit.

[2] C. J. Warden and L. H. Warner, *Quart. Rev. Biol.*, 1925, 3, 486-522

It is essential to a comparative study of behaviour that the structure of the behaviour be described in this way, but determination of structure alone is not sufficient for the study and cannot serve as its sole objective. Homology can only be established when the origins, as well as the fundamental structure of the behaviour patterns compared are alike. The criterion of homology is two-dimensional, one dimension representing the structure of the behaviour patterns at the time they are compared and the other, the historical development of the patterns. The organisation of the Symposium to include a section of 'Instincts, taxes, etc.' as separate from a section on 'Learning' emphasises the distinction between behaviour patterns which are innate and those which are acquired. The student of behaviour normally thinks of the development of these two distinct types of behaviour in terms of the processes of maturation and learning. He employs the several techniques for distinguishing between these two processes in order to determine the origin of the behaviour patterns he proposes to compare.

One of the real problems facing the comparative scientist lies in differentiating such homologous behaviour patterns from patterns which are superficially alike in structure but are not alike in origin. One example may help to illustrate the seriousness of this problem. In 1939, Maier [1] described the appearance of abnormal behaviour patterns in rats forced, with blasts of air, to respond in an insoluble problem situation. These behaviour patterns, presumably arising under conditions of conflict, appeared to fit into the classical category of 'experimental neurosis' originally described by Pavlov,[2] and were proclaimed as having great significance for neurotic behaviour in human beings. However, a more careful analysis of the origin of these behaviour patterns [3] demonstrated that they could be elicited by relatively short exposures to auditory stimulation containing the high frequency components of the air blast originally employed by Maier. The behaviour patterns observed by Maier and Pavlov were analogous but not homologous. The student of behaviour must be constantly alert to the possibility of confusions of this kind.

Among the interesting applications of the comparative method in the study of behaviour is its use in investigating neuro-physiological mechanisms associated with particular types of behaviour patterns. To Boycott and Young [4]: 'It is anomalous that we have no systematic account of the types of nervous structure that are associated with the various sorts of activity mediated by the nervous system.' Once an homologous behaviour pattern has been established as occurring in several species, a search may be instituted for those neutral structures or physiological mechanisms relevant to the behaviour in question which are common to the several species. By instituting such comparisons it is possible[1] '. . . to vary . . . the conditions of the . . . phenomena under investigation, to eliminate irrelevancies, and to discover the essentials.'

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[1] N. R. F. Maier, *Studies of Abnormal Behaviour in the Rat: the Neurotic Pattern and An Analysis of the Situation Which Produces It*, New York, 1939

[2] I. P. Pavlov, *Conditional Reflexes and Psychiatry*, New York, 1941

[3] C. T. Morgan and J. D. Morgan, *J. comp. Psychol.*, 1939, **27**, 505–508

[4] *Symposia*, op. cit.

As early as 1913 Yerkes[2] felt compelled by trends in comparative psychology to raise questions of definition. The requirements of the comparative method and its application to the study of behaviour were confused and, in the work of some researchers, they remain confused even today. The Symposium on 'Physiological Mechanisms in Animal Behaviour' should aid in clarifying some of the sources of confusion and in stimulating more careful research design in comparative studies of behaviour.

Roger W. Russell

Gedanken zur Philosophie der Mathematik, B. von Freytag gen. Löringhoff, Westkulturverlag Anton Hain, Meisenheim am Glan, 1948. Pp. 56. DM. 2.80.

Conciseness of expression cannot be claimed to be characteristic of philosophical writings. It is, however, commendably attempted in this volume which represents a somewhat modified form of two lectures given by the author in Tübingen (an English translation will be published by the Philosophical Library, New York).

The opening and the concluding sections, on 'philosophy and mathematics' and the 'human side of mathematics' respectively, are of a general non-technical character. The former, while most readable, does not contribute new findings, while the latter directs attention to what is undoubtedly a fascinating topic—that of the psychology of mathematicians. One might add to the author's generalisation about the modesty of most great mathematicians (p. 51), that there seem to be some mathematicians who do indeed exhibit no inconsiderable lack of this very desirable quality. Those interested in this section may well be referred to the late Professor Hardy's *A Mathematician's Apology*—an honest and revealing commentary on a mathematician's professional life.

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[1] J. D. Irving, *Philosophy and Phenomenological Research*, 1949, **9**, 545-557

[2] R. M. Yerkes, *J. Phil.*, 1913, **10**, 580-582

The central part of the booklet contains discussions of (*a*) the sense in which mathematical objects may be said to exist, (*b*) the meaning of ' existence ' in mathematical terminology, and (*c*) the problem of why mathematics can be applied to reality. Fairly complete answers are attempted to (*a*) and (*b*), and the author's conclusions, while not startlingly new, are arrived at after careful discussion. He finds (p. 21) that the objects of mathematics have the same sort of reality as other fictions—those occurring in fairy tales, for instance. It is characteristic of our age that the author is at pains to show that this implies no degradation of mathematics, and one might well recall that, for the Greeks, this remoteness of mathematics was just one of the reasons for its inclusion in most schemes of study. Problem (*b*) belongs to mathematics proper : a mathematical concept can clearly not ' exist ' within a framework of axioms, if its existence leads to a contradiction with these axioms.

The discussion of problem (*c*) is the most stimulating in the book, and the author arrives at the result (p. 38) that the lack of ambiguity (' Eindeutigkeit ') which is known in mathematics, can also be assumed to exist in reality, and is thus (at least in part) responsible for the applicability of mathematics. As the author stresses himself, this result is in need of development and extension. Lack of space prevents more than a bare hint as to how one might penetrate deeper into this problem. At the basis of human thought, certainly of mathematics and of language, there lies this fundamental fact, to which the reviewer has drawn attention elsewhere (*Mind*, 1948, 57, 532) : the human mind is able to abstract from individually exhibited objects arbitrarily selected qualities, so that a level of abstractions can always be found at which these objects become in fact indistinguishable. Mathematics becomes applicable to reality with the aid of mental processes of this type.

This booklet is sufficiently stimulating to be recommended, and it is to be hoped that its conciseness of style will be widely imitated.

P. T. Landsberg

Architectural Principles in the Age of Humanism, Rudolf Wittkower, The Warburg Institute, London, 1949. Pp. xii + 144. 35s.

The philosophers of science are naturally much concerned with the fifteenth and sixteenth centuries. However interesting the historical aspect may be, there stands out from it the problem of the structure of thought characteristic of the times and—equally important—the way in which it found visible shape. Consequently, we are faced with the fact of architecture, seen against the background of turmoil and strife through which men of genius gave expression to their beliefs in the great age of humanism. This is the theme of Prof. Wittkower's book. Thus it is the life-story of the outstanding theorists, Alberti and Palladio. But not of them only ; Brunelleschi, Bramante, Pacioli and others, are naturally involved.

In essence, the author's treatment is symmetrical : it begins with a general discussion of the centrally-planned church, and ends with a masterly review of the problem of harmonic proportion. In between, and very appropriately 'framed,' are chapters dealing with Alberti's approach to antiquity, and the principles of Palladio's architecture. There are two appendices concerned with matters of detail, compact and self-contained enough to be most illuminating, without detracting in the least from the clear-cut lines of the main theme.

Fundamentally, these noble Italian buildings were the outcome of the conviction that the Deity can only be conceived in mathematical terms. These terms were themselves 'given' as those deducible geometrically from the human form—the circle and the square. Proportion was taken to rest wholly upon the Pythagorean system, with little or no 'slack' left for adjustment to environment. The result was a certain aloof ruthlessness (or inevitability) which the author manages to suggest, not in so many words, but by choice of illustration and textual emphasis. Liturgically, the central church provided a number of questions, not least those concerned with the position of the altar. But it would be a mistake to infer that the humanist builders were indifferent to the claims of worship. Their God, however, was enthroned in glory—as their domes with cosmic meaning implied : He was no longer 'a man of sorrows and acquainted with grief,' as he appeared to the mediaeval creators of Gothic.

Epistemologically, all this seems to indicate a type of Neo-Platonic transcendence, valuable indeed, but bound to be demolished by subsequent movements. In his later pages, Prof. Wittkower shows how this came about. Hogarth, robust as usual, took the lead. Nevertheless, humanism produced a monumental achievement, fittingly and graciously portrayed in this scholarly book.

IAN RAWLINS

Philosophy of Mathematics and Natural Science, Hermann Weyl (Revised and augmented English Edition based on a translation by Olaf Helmer), Princeton University Press, 1949 (London : Geoffrey Cumberlege). Pp. x + 311. 40s.

PROFESSOR WEYL is one of the most eminent mathematicians of his time. Besides his distinguished contributions to pure mathematics, he is well known as one of the pioneering workers in relativity theory and as one who has played a leading part in the development of certain mathematical aspects of quantum mechanics. In all the subjects in which he has worked, he has been specially concerned with the most fundamental concepts. He has long been known as an authority also on the subjects of the present book. In fact the book itself is based upon the article ' Philosophie der Mathematik und Naturwissenschaft ' which he was asked to contribute to Oldenbourg's *Handbuch der Philosophie* published in 1927.

For the main text the author has used a translation of that article made by Dr Olaf Helmer, but with much detailed revision of the original material. To this he has added about eighty pages of appendices. He explains in the Preface his reasons for producing the book in such a fashion. Also he alludes there to the difficulties inherent in preserving in a translation the meaning of a work of this kind ; despite the evident skill bestowed upon it, these difficulties cannot but remain apparent in a number of places.

Part I of the main text is on Mathematics and consists of chapters on ' Mathematical Logic, Axiomatics,' ' Number and Continuum, the Infinite,' ' Geometry.' Part II on Natural Science has chapters on ' Space and Time, the Transcendental External World,' ' Methodology,' ' The Physical Picture of the World.' The appendices are on ' The Structure of Mathematics,' ' Ars Combinatoria,' ' Quantum Physics and Causality,' ' Chemical Valency and the Hierarchy of Structures,' ' Physics and Biology,' ' The Main Features of the Physical World ; Morphe and Evolution.'

It is hard to characterise the treatment otherwise than by the author's own statement that ' the appendices are more systematic-scientific and less historico-philosophical in character than the main text.' As is to be expected of what was originally a *Handbuch* article, the object is to expound either well-established ideas or at any rate well-recognised points of view. Naturally, it is not to develop any one particular thesis. Also, the list of headings just given will show that while the topics are not un-relateable—and nobody is better able than Professor Weyl to demonstrate their relevance to each other—they cover an enormously extended range of ideas which probably no one has yet attempted to systematise into any unified scheme. Consequently in the various sections we encounter concepts of widely different sorts and belonging to quite different levels of thinking.

The mastery shown by Professor Weyl in all these diversified fields is something to marvel at. His book is a rich storehouse of information and ideas in the majority of the most fundamental domains of thought. Perhaps, however, it could better be likened to a mansion whose treasures are all the more accessibly preserved by judicious restoration and extension of the original building rather than by the provision of a specially-built ' storehouse.'

Professor Weyl starts his treatment of mathematics in a fairly standard way with an explanation of *relations* and the principles of their combination, of mathematical *definition*, and of the *axiomatic* method. But he enriches his presentation with illuminating examples and with a wealth of historical comments. In his second chapter he soon gets to deeper levels when he points out that in the field of numbers we encounter certain basic features of *constructive cognition*. The problem of the infinite immediately presents itself (and, in fact, he subsequently suggests that mathematics may be called the *science of the infinite*). In due course he shows how this problem has been tackled by Brouwer's ' intuitive ' mathematics and by Hilbert's ' symbolic ' mathematics. Finally, the chapter on geometry is very broadly based, mainly upon ideas connected with mathematical *groups* but concluding with a section on the foundations of Riemannian geometry. The latter receives special attention because, as is well known, it provides the mathematical foundation of Einstein's theory of general relativity.

The part on natural science again starts with the presentation of standard notions, in this case on the structure of space and time, but with many penetrating critical comments. Very soon, however, we find ourselves among the deeper problems of ' subject and object ' and the ' problem of space.'

In the course of the discussion, following some remarks upon ' the conflicting philosophies of idealism and realism ' which ' signify principles of method which do not contradict each other,' Professor Weyl writes (p. 117) that 'science concedes to idealism that its objective reality is not given but to be constructed, and that it cannot be constructed absolutely but only in relation to an arbitrarily assumed coordinate system and in mere symbols . . . *the objective image of the world may not admit of any diversities which cannot manifest themselves in some diversity of perceptions* . . .' This is, of course, a generally accepted view and leads Professor Weyl on to the exposition of the broadly agreed views of science and scientific theory that have been emerging at any rate since the time of Leibnitz, though they have their origins in Greek thought. What is impressive here is that he is able to show how so many of the contributions of the great thinkers fit together into a generally coherent system. As he writes at the end of this part (p. 216), ' The more I look into the philosophical literature the more I am impressed with the general agreement regarding the most essential insights of natural philosophy as it is found among all those who approach the problems seriously . . . or if not agreement then at least a common direction in their development.'

This is reassuring. For a superficial acquaintance with the literature is apt almost to give the impression that in the history of science there has been little discernible progress in understanding its foundations and meaning. Many who write on the subject appear to start afresh for themselves. Profound as their thinking may be, it often seems to take them no higher towards the peaks of understanding than their predecessors have attained. And it does not seem to reveal any definite ledge or platform that their successors can be sure of reaching with ease and assurance in order to start upon further ascents. Only a guide with the knowledge and experience possessed by Professor Weyl can convince us that the lower slopes have in fact been fairly thoroughly explored already and that most of the serious explorations do lead to about the same place even if by various routes.

If one may exploit the simile a little further, one would say that what is now most needed is a reliable map in which the agreed results of all explorations are clearly shown. In other words, it ought now to be possible for someone to write a textbook of the principles of natural philosophy in which a number of important results could be presented with the status of ' theorems.' Actually the simile used may not be unfortunate. For a map is a symbolic representation, and it may well be that the requirement of the subject for recording its advances is now the invention of an effective symbolism.

Returning to the book itself, the chapter on methodology deals as we should expect with ' measuring ' and with the formation of concepts and theories. One is glad to see the careful attention devoted to this part of the subject. It provides the connection between experimental science and natural philosophy but its importance is often not fully recognised.

The chapter on the physical picture of the world deals with an overwhelming variety of ideas grouped under the two headings of ' matter ' and ' causality (law, chance, freedom).' Weyl includes a brief treatment of concepts of teleological origin. It is interesting that he dismisses as ' all too cheap ' any dogmatic assertion ' that purposes in life are man-made, not found.' Also he returns to this general category of ideas at the end of his fifth appendix where he says that the scientist would be wrong to ignore the possibility of ' understanding from within ' as expounded in particular by R. Woltereck. And again, evidently with similar ideas in mind, he concludes his last appendix with a thought-provoking survey, presented with all due modesty by a ' layman ' but at the same time with full awareness of current work and theory, of the unsolved problems of evolution in the large.

Professor Weyl sets special store by the lists of references which are a feature of his book and which should not pass unmentioned in any review.

The foregoing sketch is not adequate even as an indication of the scope and power of Professor Weyl's treatise. It is destined, undoubtedly, long to remain a standard work. For it is the record produced by a great mind of the hard-won ideas of many other great minds.

W. H. McCrea

The Principles of Scientific Research, Paul Freedman, Macdonald & Co. (Publishers), Ltd., London, 1949. Pp. 222. 15s.

This book is addressed to young people about to commence scientific research and to business executives. As regards the latter, it may be said at once that the style is too technical and presupposes too much specialised knowledge—to say nothing of its Marxist flavour—to appeal to them. Young people, as the author suggests, will not find it ' excessively cheerful ' : this because he not only stresses the difference between qualities necessary for successful research and those necessary for success in examinations, but also deals frankly with the human relationships inevitable in all hierarchies, including research units.

The account of scientific method is rather confusing. The author seems greatly to overestimate the present-day importance of Socratic dialogue, or dialectic (incidentally, Socrates was not the inventor of dialectic). Again, it is difficult to accept Cavendish and Pasteur as initiators of a new *principle* in science because the former aimed at the highest accuracy and the latter persisted in seeking ' experimental confirmation of an hypothesis which appears sound, in spite of repeated negative results ' ; and it is certainly curious to find Sir J. J. Thomson credited with ' the application of a new, highly important principle in scientific research—the choice between two alternative hypotheses ' (p. 56). When, further on, the reader is told of the ' utmost importance ' of the ' scientific ' principles of Marx and Engels, and the dusty, nebulous, Unity of Opposites, Negation of Negations and the rest, are paraded before him, his patience is strained, and is near to breaking, when told that ' electroplating was practised in Babylon ' (p. 85).

Although Socrates is praised for his desire to arrive at precise definitions, we are not given any definition of such key words as *truth* and *fact*. It is impossible to expound scientific method clearly without distinguishing between facts and hypotheses, and it is impossible to do this without defining them, especially the very difficult word *fact*.

The chapter on *Minimum number of essential observations* is too cursorily treated to carry conviction, but it is refreshing to find the author sceptical of the wisdom of the frequently employed practice of rejecting any value . . . which exceeds five times the ' probable error ' of the group ' (p. 190). Exception must, however, be taken to the statement ' a scientist who is, above all things, a statistician, . . .' (p. 182).

This is certainly a difficult book to appraise. In between the matter criticised above, is good advice and some helpful suggestions with regard to planning of research—but will the young research worker be able to separate the wheat from the chaff?

The book is well produced and properly punctuated (a rare virtue nowadays), and there are few slips. One slip is due to the general use of surnames only : thus, ' Young, 41, 71 ' in the index refers, presumably, to Thomas Young on p. 41, and to his living relative, Professor J. Z. Young, on p. 71.

G. Burniston Brown

Philosophic Thought in France and the United States : Essays representing major trends in contemporary French and American philosophy. Edited by Marvin Farber, University of Buffalo Publications in Philosophy, New York, 1950. Pp. x + 775. $7.50.

This bulky volume is divided into two parts. The first comprises eighteen essays by French philosophers on various aspects of modern philosophy studied in France and a concluding essay by an American on his ' reaction to the present situation in French philosophy.' Similarly, in the second part eighteen American philosophers write on philosophical studies in America and a Frenchman records his ' reflections ' on their essays. Whether the nice distinction between the American's ' reaction ' and the Frenchman's ' reflection ' is deliberate, is not indicated ! But the book suggests the question whether a specifically French or American philosophy, as distinct from the views of philosophers who happen to live either in France or in America, exists. The answer would seem to be that, although both France and America are renowned for absorbing men of widely different ethnic origins, each country has a strong national tradition which tends to be reflected in the writings of the philosophers.

In his essay on ' Philosophy of Education in France', Henri Wallon reminds us that of the two great movements in European thought at the close of the Middle Ages the Renaissance rather than the Reformation triumphed in France. He cites Rabelais and Descartes as characteristic writers. Rabelais's humanism was put into systematic form by Descartes who based his philosophy on the power of human reason. In particular, in his view the study of the physical world was made possible by man's capacity for reasoning and constructing mathematics, the key to all the sciences. Thus, the background of French education is a high *academic* tradition. In America, on the other hand, as Harold Taylor points out in his essay on ' The Philosophy of American Education,' the major concern of education has been to answer the *practical* needs of a new and growing nation. ' So practical have some universities now become that there are courses, given for academic credit in fulfilment of the Bachelor of Arts degree, in writing advertisement copy and in making one's hair and face more beautiful.' It is not surprising to find Dr Taylor complaining that those who are engaged in reforming education in America too seldom discuss the problem of ' values.' More than a hundred years ago Alexis de Tocqueville remarked that the Americans are inclined to deny what they cannot comprehend and to supply a multitude of ready-made opinions for the use of individuals who adopt them on trust. The tendency in America today, complains Dr Taylor, is to reduce all of life to a series of soluble problems and minor challenges to be overcome, with the result that aesthetic and spiritual insights are often unduly neglected.

It is therefore not surprising to find that the essay on ' Philosophy of Science in America ' by Victor F. Lenzen begins by stressing the present importance in America of the philosophical standpoint of logical empiricism. Although metaphysical factors in science were emphasised by Whitehead and Burtt during the decade after the first world war, the dominant point of view in America even then was pragmatism, the most notable contribution being Bridgman's operational theory of physical concepts which is one of extreme empiricism. On the other hand, Lenzen himself in *The Nature of Physical Theory* has sought to interpret the systematic structure of physics in the light of postulational methods and in particular has elucidated the rôle of definition. He regards ' a system of science ' as originating in certain primitive concepts which are obtained by abstraction from experience. Relations are established between them and these give rise to more precisely defined concepts. The entire structure of classical physics, according to Lenzen can thus be built up conceptually by the method of successive definition.

Turning to the French half of the book, it is disappointing to find that Professor Gaston Bachelard was ' accidentally prevented from writing ' on Philosophy of Science in France. Instead Dr Lalande has provided a most useful annotated bibliography of ' Principal Publications on the Philosophy of the Sciences brought out in France since 1900,' which runs to just over ten pages, and is divided into the following sections : General Questions, Collective Publications, Philosophy of the Mathematical Sciences, Philosophy of the Experimental Sciences, Physics, Chemistry, and Biology.

Of the other essays by French philosophers, the one likely to be of the greatest interest to readers of this *Journal* is the provocative survey of ' Logic in France in the Twentieth Century ' by Marcel Boll and Jacques Reinhart. The authors begin by pointing out that in France the word ' logic ' tends to have a somewhat wider meaning attributed to it than anywhere else. The French, they claim, have never lost sight of the fact that to be of value logic must, in the end, contribute to the further development of science. ' Consequently, in France, formal logic has hardly ever escaped the effects of a critical point of view which has probably hampered the conception of great systematizations comparable to those of Russell and Whitehead.' The outstanding French workers in this field early in this century, Louis Couturat and Jean Nicod, both died young, and after their deaths the study of logical forms appears to have stagnated. This was probably mainly due to the influence of Poincaré, one of the greatest and most prolific intuitive mathematicians (he wrote over 500 original memoirs in little more than thirty years). Regarding logistic primarily as a technique, Poincaré criticised ironically the claim of Russell and Couturat that logistic lends wings to *invention*. ' Well,' he exclaimed, pointing out that Peano had published his *Formulaire* ten years before, ' now you have been equipped with wings for ten years and you have not yet begun flying !' However, he admitted that logistic was not quite barren, for ' it has given birth to antinomies ! '

The attitude of French logicians to these notorious antinomies is far too little known in this country. For example, Poirier argued in 1938 that Epeminedes' paradox is based on ' the error of considering a contrary as contradictory,' an objection which has been sustained by Alexandre Koyré and also by Boll and Reinhart themselves. Koyré remarked that the use of symbols has hampered the investigators ' who have forfeited all critical acumen after they had clumsily transposed their subject matter into formulae.' In a detailed footnote to their essay, Boll and Reinhart similarly suggest that we should regard as a sophism the typical objection against the application of bivalent logic to microphysics. For example, in criticising Birkhoff and von Neumann (*Annals of Mathematics*, 1936) they write :

> Do not the latter go to the extreme of stating that the negation of ' the experimental observation of a train of waves Ψ on one side of a plane in ordinary space ' is : ' the observation of Ψ on the other side ?' They contradict themselves shortly afterwards when mentioning as possible ' The observation of Ψ in a state symmetrical about the said plane ' (a possibility which when contrasted with the foregoing ' premises ' manifestly leads us—through a plain Aristotelian dilemma—to an apparent antimony)—while a negation must include *all possible* cases excluded by the corresponding affirmation. The correct negation is : ' The observation of at least part of Ψ on the other side.'

At the end of their essay the authors claim that in France the existence of well-attended courses in philosophy in secondary schools helps many men of science to avoid excessive naïveté where general ideas are concerned, in particular that narrow Humean empiricism ' which seems to be the spontaneous ontology of the adult once he has set foot in a laboratory, just as Aristotelism is the ontology of a child once he has learned how to speak (an Aryan language).'

Among the American essays in this volume, ' Probability, Induction and Provident Man ' by D. C. Williams, ' Attribute and Class ' by F. B. Fitch, ' Basic Issues in Logical Positivism ' by Felix Kaufmann, may be mentioned as covering subjects within the purview of this *Journal*. Two further essays which I found of particular interest were 'The Main Trend of Social Philosophy in America' by V. J. McGill, and 'Toward an Analytic Philosophy of History' by Morton G. White. The typical defect, however, of most essays in this volume is that too much ground is covered in too few pages.

G. J. WHITROW

Louis Pasteur, Free Lance of Science, Rene J. Dubos, Little, Brown & Co., Boston, U.S.A., 1950 (published simultaneously by McClelland & Stewart, Canada). Pp. xii + 418. $5.00.

WHEN books appear on the life of an eminent scientist readers who are familiar with any biographies which have existed previously are liable to be irritated at the appearance of 'yet another book.' This is particularly the case with that doyen of the biological and medical sciences, Louis Pasteur, whose life and achievements are rewritten all too frequently by every writer of popular medical history. It is, therefore, with great pleasure that this reviewer wishes to recommend this particular book on Pasteur. Dr Dubos has not merely written another life of the great man. The book is an attractive, useful and considered study of certain biological problems up to the time of Pasteur and of the treatment which Pasteur gave to them. A full historical and methodological treatment of each topic makes the understanding of Pasteur's achievements far more real than any other account of these works which have been read.

The book can be divided into three sections. The first deals with the scientific and cultural background against which Pasteur arose, with his life, both domestic and scientific, and contains a chapter entitled 'Pasteur in action', which gives an impressive picture of his attitude and aims. This chapter with its balance between expounding Pasteur's practical methods and giving an insight to the small petty foibles that contributed to his character does more than anything else to make the great man live in the reader's mind.

The second part of the book consists of an account of the advances which were made by Pasteur in each sphere, as indicated by such chapter headings as 'From crystals to life'; 'Spontaneous generation,' 'The diseases of silkworms,' etc. The account of Pasteur's contribution is supplemented in each case by a brief but valuable historical introduction to the subject.

Finally a third part reviewing the mechanism of discoveries and giving an account of Pasteur's religious philosophy makes the book of particular interest to the philosopher of science. It is in the account of the mechanism of the discoveries that Dr Dubos brings out that the essence of Pasteur's method and success was his ability to conceive explanatory hypotheses and to work out those experiments which would be decisive before he attempted any experimental study. His genius was for the intuitive guesses from which he systematically deduced the consequences which he would have to test. I recommend this chapter to those who believe in the Baconian account of scientific method and wish to see how a great scientist really does his work (especially pages 362-363).

The style is easy to read, and demonstrates the disputes involved, the atmosphere of the laboratory and even the arrogance of a careful scientist. This is aided by liberal quotation from Pasteur's notes, letters and sayings. In addition there is a useful bibliography of works which were used and which would serve as good reading material for those interested in the history of the biological sciences.

R. F. J. Withers

The Common Sense of Science, J. Bronowski, William Heinemann, Ltd., London, 1951. Pp. 154. 8s. 6d.

Dr Bronowski considers that 'it has been one of the most destructive modern prejudices that art and science are different and somehow incompatible interests.' He believes that the proper approach to science for the non-scientist is through his previous knowledge of history, literature and social affairs. The nine chapters comprising this book are on Science and Sensibility, The Scientific Revolution and the Machine, Isaac Newton's Model, The Eighteenth Century and the Idea of Order, The Nineteenth Century and the Idea of Causes, the Idea of Chance, The Common Sense of Science, Truth and Value, Science—the Destroyer or Creator. The text is based on a series of Broadcast Talks. It is well written, the approach is clear and unsophisticated and eminently suitable for the type of reader whom the author has in mind. The index is noteworthy for consisting solely of the names with dates of about two hundred miscellaneous celebrities from J. C. Adams to Emile Zola.

G. J. Whitrow

The Origin of the Earth, W. M. Smart, Cambridge University Press, 1951. Pp. viii + 239, 8 plates. 12s. 6d.

Professor Smart's latest book, based on lectures originally given to members of the armed forces during the last year of the war, is a timely discussion for the general reader of the present state of our knowledge concerning the origin of the Earth *and* the solar system (the title is somewhat misleading). The ten chapters fall into three main parts, which the author concisely labels ' Whence ? ', ' When ? ' and ' How ? '

In the first part the author describes how the problem of the origin of the Earth has come to be regarded as subsidiary to the greater problem of the origin of the Sun's family of planets, satellites, minor planets, comets, meteors, etc. and not least of the origin of the Sun itself. He shows how the various uniformities of the solar system, in particular the fact that the planets all move in planes close to the plane of the ecliptic and in the same sense round the Sun, provide the strongest evidence that the planets have sprung from a common ancestor. The retrograde motion of a few satellites is shown to be not necessarily incompatible with this conclusion.

In the second part the author first describes the conflict in the latter half of last century between physicists and geologists concerning the age of the Earth, the geologists demanding a far longer time-scale than the physicists in the prevailing state of knowledge could provide. He then gives an account of the remarkable advance made in the present century by applying radioactive methods to the measurement of the age of the rocks, and in the final chapter of this part considers the astronomical evidence for the age of the Sun, of the Moon and of the Universe. The Bethe-Weizsäcker theory of the thermo-nuclear origin of stellar energy is clearly outlined. This theory does not include any direct estimate of the Sun's age because the original chemical composition of the Sun is unknown, but estimates can be based on ' reasonable ' assumptions concerning this composition. It appears that the Sun's age is ' almost certainly ' much less than 8000 million years. There is considerable corroborative evidence (notably that derived from the theory of the expansion of the universe) for the cosmological significance of a time-scale of a few thousand million years. In other words, we do not know how or when the Earth, Sun, stars and nebulae began, but we do know that many apparently independent lines of investigation converge to this same rough estimate of age. This internal agreement concerning order of magnitude overshadows any alleged discrepancies, e.g. that some well-known versions of the expanding universe theory imply a shorter age than that demanded by the theory of stellar energy and evolution. It is the strongest evidence we have that ' the hair-raising extrapolations ' of cosmology (to quote Bridgman's phrase) are not to be dismissed lightly as mere speculations.

The general description of the observational data and of the relevant established physical theories in the first two parts of Professor Smart's book provides the essential background to the more speculative third part in which possible mechanisms of origin are discussed. Perhaps the very plethora of modern theories of terrestrial and solar origin convinced Professor Smart that in the space at his disposal only a comparatively brief summary could be attempted. Nevertheless, within his self-imposed limits the author approaches these hypotheses in the true scientific spirit. It should, however, be mentioned that since this book went to press an entirely new approach to the problem of the origin of the planets has been made from the point of view of a *chemist.* Whatever the ultimate verdict on his particular theory, Urey's intervention may well prove to be a landmark in the history of the subject.

The production of the book is up to the high standard which we expect of the Cambridge University Press ; it is well-illustrated with photographs and is graced with a striking dust-jacket on which is reproduced a photograph of Blake's famous water-colour of the creation of the world.

G. J. WHITROW

BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

Albert Einstein, *Out of My Later Years,* Thames and Hudson, London, 1950, pp. viii + 282, 15s.

R. J. Forbes, *Short History of the Art of Distillation,* E. J. Brill, Leiden, 1948, pp. 405, 18 guilders.

R. J. Forbes, *Metallurgy in Antiquity,* E. J. Brill, Leiden, 1950, pp. 489, 19 guilders.

Edited by A. E. Heath, *Scientific Thought in the Twentieth Century,* C. A. Watts and Co. Ltd., London, 1951, pp. xv + 387, 42s.

Geoffrey Lapage, *Parasitic Animals,* Cambridge University Press; 1951, pp. xxii+ 351, 21s.

Annaliese Maier, *Zwei Grundprobleme der Scholastischen Naturphilosophie. Das Problem der Intensiven Groesse. Die Impetustheorie,* 2 Aufl, Edizioni di Storia e Letteratura, Roma, 1951, pp. x + 318.

Edoardo May, *Elementi di Filosofia della Scienza,* Fratelli Bocca Editori, Milano, 1951, pp. viii + 178, Lire 600.

Tran-Duc-Thao, *Phenomenologie et Materialisme Dialectique,* Editions Minh-Tân, Paris, 1951, pp. 368, Fr. 650.

Hans Reichenbach, *The Rise of Scientific Philosophy,* University of California Press, Berkeley and Los Angeles (London : Cambridge University Press), 1951, pp. xi + 333, 28s.

Juan Adolfo Vásquez, *Ensayos Metafisicos,* Instituto di Filosofia de la Universidad Nacional de Tucuman, Argentina, 1951, pp. 95, 8 pesos m/arg. or $1.50.

Sir Edmund Whittaker, *Eddington's Principle in the Philosophy of Science* (the fifth Arthur Stanley Eddington Memorial Lecture), Cambridge University Press, 1951, pp. 35, 2s. 6d.

Norbert Wiener, *The Human Use of Human Beings,* Eyre & Spottiswoode (Publishers) Ltd., London, 1950, pp. 241, 18s.

J. Z. Young, *Doubt and Certainty in Science* (B.B.C. Reith Lectures, 1950), Clarendon Press, Oxford, 1951, pp. vi + 168, 7s. 6d.